Essays on Household Finance and Monetary Policy

D I S S E R T A T I O N
of the University of St.Gallen,
School of Management,
Economics, Law, Social Sciences,
International Affairs and Computer Science
to obtain the title of
Doctor of Philosophy in Economics and Finance

submitted by

Benedikt Lennartz

from

Germany

Approved on the application of

Prof. Dr. Winfried Koeniger

and

Prof. Dr. Sebastian Findeisen

Dissertation no. 5230

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The University of St.Gallen, School of Management, Economics, Law, Social Sciences, International Affairs and Computer Science, hereby consents to the printing of the present dissertation, without hereby expressing any opinion on the views herein expressed.

St.Gallen, June 13, 2022

The President:

Prof. Dr. Bernhard Ehrenzeller

Contents

List of Tables

List of Figures

Acknowledgements and Summary

Writing a dissertation means an exciting endeavour but also a big challenge. Fortunately, I was surrounded by various people who supported me along the way.

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St. Gallen, September 2021

Benedikt Carl Lennartz

Summary

The dissertation consists of three chapters that analyze topics in household finance and the transmission of monetary policy. In each chapter, I find evidence for heterogeneous effects, either because households with different leverage adjust consumption after house price changes differently, or households differ in their housing tenure transitions in response to a change of monetary policy, or monetary policy transmits differently to labour market outcomes across the income distribution.

The first chapter analyzes the consumption effects of house price changes for homeowners in Italy. I use an Italian panel dataset that contains information on household incomes and balance sheets. In contrast to findings of the existing empirical literature for the US and UK, the effect of house prices on consumption is small in Italy. In line with theoretical models, I show that the effect is largest for households close to their borrowing constraint. Thus, the smaller consumption response in Italy relative to the US and UK may be explained by the lower leverage of Italian households.

The second chapter investigates the transmission of monetary policy to the housing markets in Germany, Italy and Switzerland. We identify monetary policy shocks using high-frequency data on interest rate expectations and combine it with household panel datasets. We show that monetary policy transmits to short and long term yields and mortgage rates and therefore to the user costs of housing. We estimate that interest rate cuts lead to transitions from renting to owning in Germany and Switzerland but not in Italy. Within Italy, households from financially more developed regions react more strongly by becoming homeowners, in line with a stronger effect on mortgage rates in these regions.

In the third chapter, I estimate the effect of monetary policy on labour market outcomes across the income distribution in Switzerland, a country with a high employment rate and a high level of working hours compared to other European countries. I show that, in line with findings from other countries, policy rate cuts lead to lower household income inequality. I find that for Switzerland, the

reduction of income inequality is caused by an increase of labour income and labour supply at the bottom of the household income distribution, both at the intensive and extensive margin. Further, individuals that work in more elementary occupations and in the Italian- and French-speaking regions of Switzerland gain more from interest rate cuts.

Zusammenfassung

Diese Dissertation besteht aus drei Kapiteln, die sich mit den Themenbereichen Haushaltsfinanzen und Transmission von Geldpolitik beschäftigen. In jedem der drei Kapitel finde ich Evidenz für heterogene Effekte, sei es wenn Haushalte mit unterschiedlichem Verschuldungsgrad ihren Konsum nach einer Hauspreisveränderung unterschiedlich anpassen, oder Haushalte ihre Wohnbesitzverhältnisse als Reaktion auf geldpolitische Änderungen wechseln, oder sich Geldpolitik unterschiedlich auf Arbeitsmarktergebnisse entlang der Einkommensverteilung überträgt.

Das erste Kapitel analysiert die Konsum-Effekte von Hauspreisveränderungen für Hauseigentümer in Italien. Hierzu nutze ich einen italienischen Panel-Datensatz, der Informationen zu Haushaltseinkommen und Vermögensverhältnissen enthält. Im Gegensatz zu den Befunden aus der Literatur für die USA und das Vereinigte Königreich ist der Effekt von Hauspreisen auf den Konsum in Italien klein. In Übereinstimmung mit theoretischen Modellen kann ich zeigen, dass der Effekt am grössten für Haushalte in der Nähe der Beleihungsgrenze ist. Somit könnte die geringere Reaktion des Konsums in Italien verglichen mit den USA und dem Vereinigten Königreich durch die niedrigere Verschuldung der italienischen Haushalte erklärt werden.

Das zweite Kapitel untersucht die geldpolitische Transmission auf die Immobilienmärkte in Deutschland, Italien und der Schweiz. Wir identifizieren geldpolitische Schocks mittels Hochfrequenzdaten zu Zinserwartungen und verbinden diese mit mit drei Haushaltspanels. Wir zeigen, dass sich Geldpolitik auf kurz- sowie langfristige Renditen und Hypothekenzinsen und somit auf die Nutzungskosten von Wohneigentum überträgt. Wir schätzen, dass Zinssenkungen zu Wechseln von Wohnbesitzverhältnissen von Miete zu Hauseigentum in Deutschland und der Schweiz führen, jedoch nicht in Italien. Innerhalb Italiens reagieren Haushalte aus finanziell stärker entwickelten Regionen mehr, indem sie Hauseigentümer werden, entsprechend einem stärkeren Effekt auf die Hypothekenzinsen in diesen Regionen.

Im dritten Kapitel schätze ich den Effekt von Geldpolitik auf Arbeitsmarktergebnisse entlang der Einkommensverteilung in der Schweiz, einem Land mit einer hohen Erwerbsquote und einem hohen Niveau von Arbeitsstunden verglichen mit anderen europäischen Ländern. Ich zeige in Übereinstimmung mit Befunden zu anderen Ländern, dass Zinssenkungen zu niedrigerer Einkommensungleichheit zwischen Haushalten führt. Ich finde heraus, dass in der Schweiz der Rückgang der Einkommensungleichheit durch eine Erhöhung der Arbeitseinkommen und des Arbeitsangebots im unteren Teil der Haushaltseinkommensverteilung hervorgerufen wird, sowohl an der intensiven als auch an der extensiven Marge. Zudem profitieren Individuen, die in einfacheren Tätigkeiten arbeiten und in den italienisch- und französischsprachigen Regionen der Schweiz, stärker von Zinssenkungen.

1 The Consumption Effects of House Price Changes: Evidence from Italy

Abstract:
I estimate the house price elasticity of consumption with individual household data from the Italian Survey on Household Income and Wealth (SHIW). The structure of the data set allows to control for otherwise omitted variables such as household income growth. In contrast to most of the existing empirical literature, I control for region, year and region-year fixed effects to mitigate endogeneity concerns. In the baseline model, a 1% increase in house prices is associated with a 0.05% increase in homeowner consumption expenditures, an estimate below typical estimates for the US of around 0.25%. I provide evidence that the smaller house price elasticity of consumption in Italy is associated with less household leverage, consistent with the collateral channel.

JEL-Classification: E21, D14, R21
Keywords: consumption, house prices, leverage

1.1 Introduction

Do households adjust their consumption expenditures when they experience a change in their house value? This question is a matter of discussion particularly since the Great Recession. That recession was characterized in many developed countries by the stop of the housing boom and a large fall of house prices. At the same time, households reduced their consumption expenditures. In the US, Beraja et al. (2019) show that the regions with the largest fall in house prices were also those with the largest increase in unemployment rates.

The responsiveness of household consumption to house prices is of relevance for both fiscal and monetary policy. As Beraja et al. (2019) point out, a fall in house prices makes households less likely to refinance in response to a mortgage rate decline. Similarly, households' response to fiscal policy depends on their balance sheets and the valuation of their balance sheet positions in turn depends on house price movements (Kaplan and Violante, 2014).

The theoretical literature has identified two main channels through which house prices may directly affect consumption. The first channel is the *wealth effect*. Given the assumption that house price changes are not transitory, a positive house price shock can be seen as a permanent increase of a household's wealth. If households plan to sell their property, they may wish to consume some of the expected gain, so consumption should increase (Jappelli and Pistaferri, 2017) . If households, however, do not plan to sell their house, the wealth gain from an increase in their house prices would never materialize and therefore also not affect consumption (Attanasio et al., 2009; Campbell and Cocco, 2007; Sinai and Souleles, 2005).

The second channel is the *collateral effect*. An increase of house prices relaxes the borrowing limit for households, because housing is used as collateral for mortgages. Households who are at their borrowing constraint can refinance their mortgages and thereby extract home equity for consumption (Campbell and Cocco, 2007). The house price elasticity varies across households depending on their balance sheets. Households with large debt levels, i.e. close to their borrowing limit, should react more strongly to changes in their house prices (Berger et al., 2018).

Another theoretical explanation for the empirically observed positive correlation between consumption growth and house price growth is the omission of a third variable causing both. According to Attanasio et al. (2009), income expectations are a candidate for this third variable. They argue that higher income expectations should increase consumption because households try to smooth consumption over the life cycle. If housing services and consumption are complements in the utility function, the demand for housing services should increase and house prices should increase as well. I find that controlling for expectations on next year's income does not change the size of the estimated elasticity.

In this paper, I estimate the size of the effect of house prices on consumption for homeowners and the heterogeneity of the effect using data from the Italian Survey on Household Income and Wealth (SHIW). The survey data is well-suited for this purpose for the following reasons. The SHIW is one of the few panel data sets that tracks information on household consumption, income and balance sheets over time. It allows to control for otherwise omitted variables such as (expectations on) household income that may bias the elasticity estimate. Further, I can identify house price changes at the individual level. It shall be noted that the house price estimates used in this paper are self-reported. I show that they on average move in line with official house price statistics.

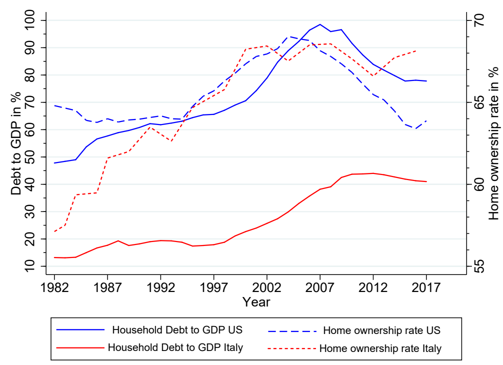
This allows to study the house price elasticity of consumption across other household characteristics. Specifically, I analyze the effects of house prices on consumption for households with different debt levels and age structures.

Many studies that measure the effect of house prices on consumption have to rely on regional variation in order to identify the effect, introducing the possibility that an unobserved variable, that varies at the regional level, biases the elasticity estimate. The SHIW allows to control for region, year and region-year fixed effects. I show that introducing those fixed effects reduces the estimated house price elasticity of consumption.[1] Using data from the survey years 1989 and 1991, I also find that income expectations, a main candidate in the literature for an omitted variable that biases the estimate, do not drive both consumption and house prices at the sub-regional level. This result provides further evidence against the existence of sub-regional confounders.

Furthermore, the structure of the balance sheets of Italian households is different compared to the US, which has been analyzed in much of the research on the topic. The Italian survey data allows to analyze how these differences in balance sheets affect the house price elasticity. Figure 1.1 shows the household debt to GDP ratio and the home ownership rate for the US and Italy. Both countries have high home ownership rates, so changes in house prices affect a large fraction of the population. In 2016 the home ownership rate in Italy was around 68%, so almost 5 percentage points higher than in the US. At the same time, households in the US are more indebted than Italian households with an aggregate debt to GDP ratio that is almost 40 percentage points lower in Italy.

Given the theoretical findings on the effect of house prices on consumption discussed before, we may expect the aggregate effect to be smaller in Italy. There are fewer households at their borrowing constraint so the collateral effect should not apply to many households. Furthermore, in 2016 around 30% of main residences recorded in the SHIW were inherited. If many households do not plan to resell their houses, the wealth effect should also be less important.

[1] As Nakamura and Steinsson (2018b) point out, obtaining *relative* effects facilitates identification in macroeconomic settings. One should, however, note that the relative estimates reported in this paper are obtained by comparing consumption responses from different house price changes at the individual level. It follows that they cannot be interpreted as aggregate effects.



Sources: Household debt to GDP ratios retrieved from the BIS (BIS long series on total credit, borrowers: Households & NPISHs, Lenders: All sectors, Valuation: Market value, Unit type: Percentage of GDP), home ownership rate for the US retrieved from the FED (FRED code: *RSAHORUSQ156S*), home ownership rate for Italy calculated with the SHIW. *Notes:* For Italy, the graph shows the fraction of households that own the house they live in (owner occupation rate).

Figure 1.1: *Household debt and home ownership in the US and Italy*

In line with the theoretical predictions, I find that the house price elasticity of consumption in the sample of all homeowners is significant but smaller for Italy than the US. Specifically, a 10% increase in house prices is associated with a 0.5% increase in consumption.

With respect to the collateral effect, I find that the estimated elasticity increases with household indebtedness. Loan-to-value ratios comparable to those observed in the US result in an extrapolated elasticity estimate of around 24% – 30%, depending on the specification. I find that those households with loan-to-value ratios at the borrowing limit (around 80% of the house value) exhibit the largest house price elasticity of consumption, in line with the theoretical literature. The fact that Italian households have relatively less debt and few households at the borrowing constraint may therefore explain the lower average effect compared to studies using data from the UK or US.

With respect to the wealth effect, I do not find that the house price elasticity of consumption varies significantly with the age of the household head. The theoretical literature would suggest that older households react more strongly to house price changes since their remaining life cycle is shorter.

The growth rate of consumption, which is the dependent variable in the main regressions, has a lower bound at -1. The linear baseline specification could predict consumption growth rates below that lower bound. As Christelis et al. (2015), in the robustness I therefore use an ordered probit model and find that a 15 percentage point increase in house prices is associated with a 0.5 percentage point increase of the probability that consumption increases, although the marginal effects are insignificant. The marginal effect of house prices again increases with household indebtedness. For households with loan-to-value ratios greater than 50%, for instance, the same increase in house prices is associated with a five percentage points increase of the probability that consumption increases.

This paper proceeds as follows. Section 1.2 presents an overview of the existing literature on the effect of house prices on consumption. In Section 1.3, I present the data from the Italian household survey. Section 1.4 summarizes the main results and a discussion of remaining confounders that may bias the estimate. In Section 1.5, I provide evidence on the heterogeneity of the effect with respect to household indebtedness and age. In Section 1.6, I provide robustness checks. Section 1.7 concludes.

1.2 Literature

While the theoretical literature agrees that the effects of house prices on consumption is positive, they disagree about the size of the effect (Jappelli and Pistaferri, 2017). In a recent contribution, Berger et al. (2018) investigate the house price elasticity of consumption in a heterogenous agent life-cycle model with housing choice. In their baseline model, they find an elasticity of around 0.3, so their model reproduces recent empirical estimates for the US. They also find that the house price elasticity of consumption should increase with household leverage.

Hintermaier and Koeniger (2018) calibrate a life-cycle incomplete markets model to different euro zone economies. For Italy, they show that a 10% fall of house prices leads to a 2% decrease of aggregate consumption, an estimate at the lower bound of US estimates.

Many empirical studies discuss the link between wealth changes, specifically house price changes, and consumption. Due to data availability, most of these studies focus on the US and UK. Some estimate the effect of house prices on consumption at the aggregated level. So both house prices and consumption are measured for instance at the country, state or ZIP code level.

Carroll et al. (2011) use aggregate wealth and consumption data from the US and find that the initial effect of house prices on consumption (in the same quarter) is considerably smaller than the long-run effect. This result suggests that observing house price and consumption data every two years as in this paper may actually be an advantage in order to capture the full effect. Using data for 14 advanced countries as well as US states, Case et al. (2005) find a positive effect of housing wealth on consumption but a much weaker effect of financial wealth.

Mian et al. (2013) use regional variation in wealth losses due to house price changes in the US to estimate the consumption response to these changes. Their identification relies on regional data. They find large marginal propensities to consume out of housing wealth. They use regional credit card and car sales data to estimate consumption responses at the county level. In order to identify exogenous variation in house prices given that those areas with strong exposure to the recession (for instance because of local industry composition) may have also experienced large drops in property prices, they use the housing supply indicator constructed by Saiz (2010) as an instrument for house price growth. Mian et al. (2013) find a very large elasticity of consumption with respect to housing net worth of 0.6 to 0.8.

Kaplan et al. (2020a) use regional retail sales data of non-durable consumption and house price data from an online portal to estimate the elasticity of consumption with respect to house price changes during the Great Recession in the US. They find an elasticity of non-durable consumption expenditures with respect to housing net worth of 0.24 in an OLS-specification and 0.36 using an IV approach.

Using regional variation in house prices to explain regional variation in consumption entails the problem of potential endogeneity. Regionally, an increase in house prices may be caused for instance by differing industry compositions which may also directly affect consumption on the regional level (Mian et al., 2013). One way to try to address this concern is to use an instrument to predict house price growth at the regional level. The housing-supply instruments used in the literature is a matter of discussion, because it is likely correlated with demand factors such as productivity growth (Davidoff, 2016).

Guren et al. (2021) account for these concerns by developing an instrument that captures the local sensitivity to regional house prices. Comparing their housing wealth elasticities to the ones obtained with OLS and controlling for region-time fixed effects and to the instrument from Saiz (2010) shows that the estimates are similar across specifications. The elasticities for the US range from 0.055 for the sensitivity instrument for the period 2000-2017 to 0.141 for the Saiz instrument for the period 1990-2017. Guren et al. (2021) also argue that higher leverage does not necessarily increase the house price elasticity of consumption, since households with very low loan-to-value ratios, because they are far away from their borrowing constraint, and very high loan-to-value ratios, because they are already under water, should not react strongly to house price changes. I show that the house price elasticity of consumption is highest around the borrowing constraint, in line with the predictions from Guren et al. (2021).

Campbell and Cocco (2007) estimate the effect of regional variation in house prices on individual consumption in the UK. They use individual variation in the British Family Expenditure Survey (FES) to find that house price elasticity of consumption varies with age and tenure status. For old home owners they estimate it to be as large as 1.7 while for young renters they estimate an elasticity that is not significantly different from zero. They also control for income changes.

Attanasio et al. (2009) use individual expenditures from the FES and regional house prices to estimate the association of house prices and consumption in the UK. They find a strong and stable positive association between the two, which they attribute to common causality, mainly revised expectations about future income. They estimate the largest effect for young households, which they interpret as evidence that it is not the wealth effect that is at play. The wealth effect should be larger for older households because they will sell or bequeath their house in the nearer future.

Both Campbell and Cocco (2007) and Attanasio et al. (2009) use consumption data at the household level, which allows them to control for household characteristics and household-specific variation of income. Since they identify the effect of house prices without controlling for regional variation, unobservable differences at the regional level may confound their results.

A further strand of the empirical literature uses individual variation in house prices and consumption. Cloyne et al. (2019) use individual administrative data in the UK and estimate that a 10% increase in house prices leads to a 2% increase in borrowing. Christelis et al. (2015) use individual data from the Health and Retirement Survey (HRS) in 2008 and 2009 in the US. They find that a 10% decrease in the value of the main residence is associated with a 0.54% decrease in consumption expenditures. Converted into a yearly elasticity in order to compare the estimates to this paper's findings, their elasticity estimate is 0.23.

Surico and Trezzi (2019) find, using the 2010 and 2012 waves of the SHIW, that households with debt significantly reduce durable consumption expenditures in response to the introduction of a property tax. They find that a €100 increase in the value of the house is associated with a €0.97 increase of non-durable consumption and no significant change of durable consumption, which is close to this paper's baseline estimates.[2] Paiella and Pistaferri (2017) find using information from the 2008 and 2010 waves of the SHIW that a €100 increase in *expected* housing value is associated with a €3 increase in consumption. Grant and Peltonen (2008) use information from the 1989-2002 waves of the SHIW and estimate a house wealth

[2]The mean real house price in the survey years 2010 and 2012 is €243,868, mean real consumption is €20,443. At the mean, the estimates corresponds to an elasticity of 12%.

elasticity of consumption of 8%.[3] Guiso et al. (2006) use the 1991-2002 waves of the SHIW to construct housing capital gains using detailed information on house prices at the provincial level and individual levels of housing wealth. For the sample of homeowners, they estimate a marginal to propensity to consume out of housing wealth of around 3%.[4] Because they identify house price growth at the provincial level, Guiso et al. (2006) do not control for region and region-year fixed effects.

This paper adds to the existing literature in at least two ways. First, I estimate the house price elasticity of consumption using household data from Italy. Most of the existing studies focus on the US or UK. Given the particular balance sheet structure of Italian households (low debt and high home ownership) the estimates can inform the discussion about why the estimated elasticities vary across countries. Second, I identify the effect of house prices on consumption at the individual rather than regional level. Therefore, no unobserved regional circumstances that are correlated with regional house prices and consumption, bias the elasticity estimate. The data set allows to control for (regional) year fixed effects and thus allows to control for different macroeconomic conditions across regions.

1.3 Data

The SHIW provides data on household characteristics, balance sheets and incomes publicly available mostly bi-annually since 1977. Each survey year around 8,000 participants were covered. I only consider households that have a house. I use the variation in house price growth rates across households to identify the effect of house prices on consumption. All monetary variables used in this paper are deflated using the deflator provided by the Banca d'Italia.

The structure of the data set allows to control for household characteristics such as income changes and financial asset changes. As described before, most studies

[3] Grant and Peltonen (2008) do not identify houses by construction year, which makes it likely that relocation drives part of the observed changes in house prices.
[4] This marginal propensity to consume translates to an elasticity of close to 36%.

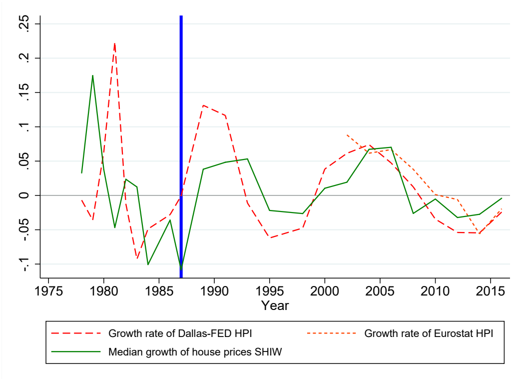
that estimate the effect of house prices on consumption either rely on regional data or study a particular time period. Using regional variation in house prices and consumption entails the potential problem that a third confounding variable at the regional level causes both house prices and consumption to change. Using the SHIW data set allows to control for regional variation. Other studies only focus on a specific time period (e.g. the Great Recession). This approach, however, makes it impossible to control for the macroeconomic environment, for instance by including time fixed effects. Using the SHIW data set, I control for regional as well as time variation and identify the effect of house prices on consumption using the variation in house price growth rates across individual households. In the following, I describe the variables used and how they are obtained.

1.3.1 House prices

In the SHIW, households are asked to provide an estimate for the value of the house they occupy. Specifically, they are asked: *In your opinion, how much is your house/flat worth (unoccupied)? In other words, what price could you ask for it today (including any cellar, garage or attic)? Please give your best estimate.*

I use the household-specific time variation of this estimate to calculate house price growth. Since the data are not available on a regular basis, I assume a constant yearly growth rate in order to annualize the growth rates according to the following formula: $\frac{\Delta H_{i,t}}{H_{i,t-1}} = \left(\frac{h_{i,t}}{h_{i,t-n}}\right)^{\frac{1}{n}} - 1$, where $h_{i,t}$ is the reported real house price of household i at time t that I deflate using the price deflator provided by the Banca d'Italia (2018). $h_{i,t-n}$ is the reported and deflated house price of household i at the last survey date $t-n$.[5]

[5] This procedure allows to pool all survey years irrespective of the time lag between the respective years. For 1986-1987 there is one year between survey waves and for 1995-1998 there are three years.



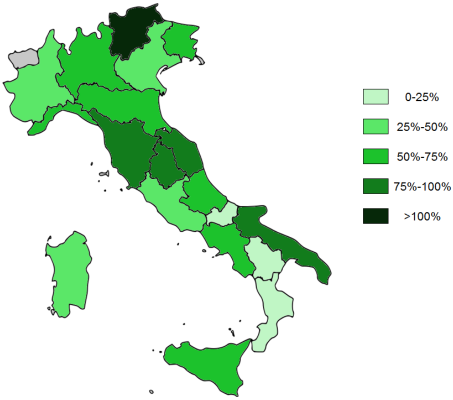
Sources: Dallas-FED HPI data retrieved from Mack et al. (2011), Eurostat (House price index - annual data, code: *tipsho20*), SHIW. *Notes:* From 1987 onwards, the house price changes obtained from the SHIW are calculated using construction year as identifier.

Figure 1.2: *Comparison of HPI growth rates*

In order to identify valuation changes of a housing unit, I only consider households that have stayed in the same unit. That is, I consider households that, over subsequent survey years, have stayed in housing units with the same construction year. This reduces the sample, because information on the year of home construction is only available from 1986 onwards.

Figure 1.2 compares the resulting series with other existing benchmarks for house prices at the aggregate level, i.e. the median of $\frac{\Delta H_{i,t}}{H_{i,t-1}}$ in a given year t. For the years after 1986 I only consider houses with the same construction year over time. For the years up to 1986 I use the median growth rate based on all available house prices. The remaining two lines represent the yearly growth rates of the official Italian house price index from Eurostat and the yearly real growth rate of house prices for Italy calculated by Mack et al. (2011) available at the FED. This house price index is calculated using data from the Italian research institute ”Nomisma”.

Before 1986 the FED index and the measured house prices are negatively correlated with a correlation coefficient of -0.2. Only after 1986, when we can identify valuation changes better in the SHIW as explained above, both series move in line and are positively correlated with a correlation coefficient of 0.6. The visible lag is the result of the bi-annual survey structure, and thus perfect correlation cannot be obtained. Figure 1.2 shows that the median growth rate of house prices in the SHIW is very similar to the growth rate of the official house price index from Eurostat (2019) with a correlation coefficient of 0.7. Overall, this evidence suggests that households' valuation of their occupied housing units are remarkably similar on average to official statistics.



Sources: SHIW. *Notes:* The figure shows the the growth rate of the median real house price per square meter from 1986-1991 until 2010-2016. Aosta Valley is omitted because of the insufficient number of observations.

Figure 1.3: *Growth rate of median real house price per m^2 from 1986-1991 until 2010-2016*

In order to investigate the regional patterns of house price growth, Figure 1.3 summarizes the growth rate of median real house prices per square meter from 1986-1991 until 2010-2016 in the different Italian regions. I use time intervals to ensure that there are at least five observations per region to calculate the median house price per square meter. House prices grew most in Trentino-South Tyrol and the central Italian regions Tuscany, Umbria and Marches. The Southern Italian regions Calabria, Basilicata and Molise were those with the lowest increase in house prices. These regions are also in the bottom third of Italian regions ranked by GDP per capita (see Table 1.9). At the same time the regions with high house price growth seem to be more touristic, which makes them attractive for foreigners to acquire homes.

House prices over the sample period feature substantial regional variation. In those regions with the highest house price growth they doubled in real terms, while in those regions with the lowest house price growth they hardly increased. The data suggest that on average those Italian regions with a stronger economy were those with higher house price growth. It is therefore important to control for regional developments when identifying the effect of house prices on consumption.

The importance of controlling for region and region-year fixed effects is also suggested by the findings from Guiso et al. (2004), who document regional differences in credit supply. They show with data from the SHIW that there exist substantive differences in access to credit across Italian regions, and Italian regions with easier access to credit tend to be in the North.

1.3.2 Consumption

For consumption, I use the amount of real expenditures on durables and non-durables (excluding expenses for maintenance of property, rents and real goods), denoted as $c_{i,t}$. I exclude these expenditures, because consumption should not include any investment in property.[6] As for house prices, I define: $\frac{\Delta C_{i,t}}{C_{i,t-1}} = \left(\frac{c_{i,t}}{c_{i,t-n}}\right)^{\frac{1}{n}} - 1$. Figure 1.4 shows the yearly median growth rate of real consumption expenditures since 1987 and the median growth rate of real house prices discussed before. For the majority of years median consumption growth was negative. Mean consumption growth (not shown), however, was mostly positive. Except for the first sample years, house price growth and consumption growth are positively related over time. The correlation coefficient for the sample years

[6] In section 4.3, I consider the effect of house prices on non-durable and durable consumption separately.

after 1987 is 0.2. Given that this positive association may be driven by changes in macroeconomic conditions, I control for year fixed effects in the regression.

1.3.3 Other variables

The SHIW provides data on the value of financial assets. The value of financial assets of a household can increase for two reasons. First, there could be an increase in the price of the financial asset held by the household. Second, the household could decide to accumulate additional financial assets. In other words, a change in the value of financial assets can happen because of a volume change, a price change, or a combination of the two. Thus, the growth rate of the value of financial assets does not only capture the change of financial asset prices since the last survey year, but also accumulated savings. Therefore, I approximate the growth rate of the value of financial assets by income from financial assets relative to financial assets. This approximation assures that savings are not interpreted as capital gains.

The income growth rate is defined as the percentage change of net disposable income excluding income from financial assets. Income growth is annualized in the same way as house price and consumption growth. The household finance literature suggests that consumption should be increasing with age, so the age of the household head is also included in the regressions. I also control for the change in the number of family members and for the fact that the household head may become unemployed.

Sources: SHIW. *Notes:* The figure shows the median growth rates of real house prices and real consumption expenditures in a survey year.

Figure 1.4: *House price growth rates and consumption growth rates*

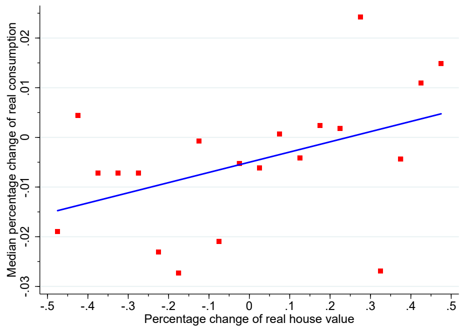
Table 1.1: *Descriptive statistics*

	Mean	Median	SD	Min	Max
Percentage change of real consumption exp.	0.04	-0.00	0.31	-0.84	5.47
Percentage change of real house value	0.04	-0.01	0.26	-0.81	4.70
Percentage change of real income	0.03	0.00	0.32	-0.87	7.66
Percentage change of real financial assets	-0.02	0.02	0.16	-1.47	1.02
Age	57.27	57	14.26	17	101
Change in number of family members	-0.05	0	0.47	-4	6
Became unemployed	0.01	0	0.10	0	1
Observations	10,591				

Sources: SHIW. *Notes:* Descriptive statistics for observations used in the baseline regression. Years 1987-2016.

Table 1.1 summarizes the descriptive statistics for the variables included in the baseline model. The median rates of growth of consumption, house value, income and financial asset are all close to zero. The table further shows that there is substantial variation of these growth rates across households. Note that the variables are not expressed in adult equivalents so that changes in household size also contribute to the growth rates, explaining some of the high consumption growth rates. I control for the change in the number of family members in the regression. As described above, only observations starting in year 1987 are included in order to calculate house price growth based on the same housing units over time.

1.3.4 Descriptive analysis



Sources: SHIW. *Notes:* Median consumption growth by 5% bins of reported property value growth. Dots are in the bin middle. $y = 0.021^{**}x - 0.005$, $R^2 = 0.16$

Figure 1.5: *House price growth rates and consumption growth rates*

Using data on individual house price dynamics, we first observe the correlation with individual consumption changes. Figure 1.5 provides the median rate of consumption growth for 5% bins of house price growth rates together with a linear regression line. In many ways this graph resembles Figure 2 in Christelis et al. (2015), although I provide the median instead of mean of consumption growth. Without accounting for additional variables, a 10% increase in house prices is associated with a 0.2% increase in consumption expenditures. The coefficient is significant at the 5% level.

My identification strategy relies on individual variation in both consumption and house prices. Therefore, I can control for region, year and region-year fixed effects. Thus, I use the differences of house price growth for households in a given region in a given year. Figure 1.9 in Appendix 1.8.1 shows a histogram of the standard deviations of house price growth rates after controlling for region and year fixed effects. On average, the standard deviation is at 0.24 indicating that there is substantial variation in house price growth across regions and survey years.

1.4 Results

1.4.1 Main results

A convincing identification of the causal effect of house prices on consumption needs to control for possible confounding factors. There are a number of observables that could be correlated with consumption and house price growth. Candidates for these omitted variables are income growth and financial asset growth. Further, it might be that the observations in the upper right part and lower left part of Figure 1.5 simply correspond to different phases of the business cycle or different Italian regions.

In order to identify the effect of house prices on consumption one needs to control for potentially omitted variables that are both correlated with both consumption and house price growth. I build on the existing consumption literature to obtain the following linear model, as found for instance in Jappelli and Pistaferri (2017), and estimate:

$$\frac{\Delta C_{i,t}}{C_{i,t-1}} = \alpha + \beta \frac{\Delta H_{i,t}}{H_{i,t-1}} + \gamma \mathbf{K_{it}} + \varepsilon_{it}. \tag{1.1}$$

$\frac{\Delta C_{i,t}}{C_{i,t-1}}$ is the household-specific growth rate of real consumption expenditures at time t and $\frac{\Delta H_{i,t}}{H_{i,t-1}}$ denotes the household-specific growth rate of the real house price at time t. The vector $\mathbf{K_{it}}$ summarizes household-specific growth rates of real income and financial assets at time t. So I allow the consumption response to real asset prices (house prices) to differ from the response financial asset prices. As Browning and Lusardi (1996) point out, in a log-linearized Euler equation the vector $\mathbf{K_{it}}$ shall account for other factors that affect households' utility, such as demographics or household size. Therefore, in the refined baseline specification I include age and household size in $\mathbf{K_{it}}$. In order to make the estimates comparable to those provided by Christelis et al. (2015), I also add a dummy that captures the transition into unemployment into the control vector $\mathbf{K_{it}}$.

Equation (1.1) is almost identical to that estimated by Christelis et al. (2015). Any household-specific fixed effects that could affect consumption expenditures cancel out because of the use of first differences. Adding household-specific fixed effects to a regression in growth rates and not in levels is not supported by theory and also would reduce a lot of the variation in house price growth since a lot of households are only observed twice. In the regression below, I also add year-, region- and year-region fixed effects to equation (1.1) in order to check the robustness of the estimated effect.

Table 1.2 provides the results of the baseline model.[7] The first column shows the elasticity estimate when only controlling for financial asset growth and income growth. In this regression I do not control for year or region fixed effects. The estimated house price elasticity of consumption is 9.6% and significantly different from zero at the 10% level.

The second column additionally controls for the age of the household head, a change in the number of family members and the transition to unemployment. The elasticity estimate does not change by much and is still significant at the 10% level. Further, the estimated income elasticity of consumption is lower. Also note that a change in the number of family members is associated with higher

[7] Table 1.10 in Appendix 1.8.2 shows the main results with standard errors clustered at the regional level as a robustness exercise. I decide not to cluster at the regional level in the baseline regression, because house prices vary at the individual level.

consumption growth. This finding suggests a reason, why the income elasticity declines from the first to the second column.

The third column additionally controls for the 20 Italian regions. As shown before, there is large regional variation in house price growth over the sample horizon. One might be concerned that regional house price growth is correlated with unobserved regional economic conditions that also affect consumption. The elasticity estimate shown in the third column is robust to the inclusion of regional dummies with a point estimate of 9.5%, which remains significant at the 10% level.

In the fourth column, I additionally control for year fixed effects. As shown in Figure 1.4, median house price growth and consumption growth are correlated over time, which suggests that they are both also determined by macroeconomic conditions. Including year dummies reduces the elasticity estimate to 8.1% with a significance at the 5% level.

The fifth column adds region-year fixed effects to capture potential regional business cycles that simultaneously affect house price growth and consumption growth. The estimated house price elasticity of consumption reduces to 5.2% due to the inclusion of these time-varying regional fixed effects. The elasticity estimate remains significant at the 5% level. These baseline results suggest that controlling for time-varying regional fixed effects is important for the size of the elasticity estimate.[8]

Overall, I find a low positive association of house price growth and consumption growth. In the baseline specification with a 10% increase in house prices is associated with a 0.5% increase of consumption expenditures. The effect is robust to adding household-specific controls, region and year dummies. The estimated elasticity is smaller than most estimates in the literature for the US and UK. One reason for the smaller size may be the structure of household balance sheets as shown below.

[8] One might be concerned that renovations done by households are an omitted variable in the main specification. Restricting the sample to households that have made at most €1000 renovation expenditures does not change the elasticity estimate in a meaningful way nor the significance level.

Table 1.2: *Baseline results*

	(1)	(2)	(3)	(4)	(5)
Percentage change of real house value	0.096*	0.094*	0.095*	0.081**	0.052**
	(0.052)	(0.050)	(0.050)	(0.052)	(0.020)
Percentage change of real financial assets	0.015	0.035	0.033	0.030	0.026
	(0.029)	(0.030)	(0.029)	(0.028)	(0.027)
Percentage change of real income	0.392***	0.354***	0.353***	0.300***	0.270***
	(0.128)	(0.112)	(0.112)	(0.079)	(0.037)
Age/100		-0.039	-0.036	-0.048	-0.044
		(0.036)	(0.037)	(0.033)	(0.030)
Change in number of family members		0.091***	0.091***	0.078***	0.062***
		(0.030)	(0.030)	(0.020)	(0.010)
Became unemployed		0.025	0.028	0.027	0.029
		(0.041)	(0.041)	(0.041)	(0.039)
Constant	0.021***	0.049**	0.051	0.073*	0.568***
	(0.005)	(0.022)	(0.031)	(0.043)	(0.071)
Observations	10,591	10,591	10,591	10,591	10,591
Adj. R^2	0.18	0.19	0.20	0.26	0.35
Region dummies	No	No	Yes	Yes	Yes
Year dummies	No	No	No	Yes	Yes
Region-year dummies	No	No	No	No	Yes

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. Robust standard errors are displayed in parentheses below the coefficients.

The estimated elasticity of consumption with respect to the value of financial assets is also positive in all of the specifications. The point estimate is slightly (although not significantly) smaller than the one for house prices, which suggests that consumption growth of Italian homeowners responds to house price gains as well as financial asset gains.

Furthermore, there is a strong and significant association between income growth and consumption growth. The size of the point estimate of the elasticity is around 4 to 5 times as large as the house price elasticity estimate. An interpretation of the size of the income effect is not straightforward, because the duration of the income shock is unknown. The age of the household head is negatively associated with consumption growth. This finding is in line with predictions from the consumption literature, which finds that consumption is ususally hump shaped over the life cycle. Becoming unemployed does not significantly affect consumption growth and the estimated effect is positive. This finding may be due to the fact that income changes already capture effects of changes in the employment status.[9]

I now turn to the issue of potential endogeneity, which is the main challenge that the literature on the consumption effects of house prices deals with. As shown above, the identification of house price growth rates at the household level allows to control for macroeconomic conditions at the regional level, which reduces the correlation coefficient. The reduction of the estimate, when region-year fixed effects are introduced, suggests that there are regional unobservables that affect both house prices and consumption in some survey years. The results suggest that not controlling for these unobservables leads to an overestimation of the coefficient of interest, which is a concern for results reported in the existing literature.

In the regression specification which controls for region-year fixed effects instead, any remaining omitted variable bias can only be at work at the sub-regional level and has *not* to be accounted for by the controls for individual income growth or financial asset growth. In the following, I provide evidence against the existence of sub-regional confounders. I use information from two waves of the SHIW to control for income expectations, a potential confounder discussed for instance by Attanasio et al. (2009). The elasticity estimate does not change when I introduce add income expectations on the right hand side of the regression. Table 1.15 in Appendix 1.8.5 also provides estimates for the baseline effects for the sample of renters. The estimated house price elasticity of consumption is not significantly different from zero. One would indeed expect that the effect on renters should be lower, because not all renters expect to inherit a house in the future.

1.4.2 Controlling for expected income changes

A common criticism to the causal interpretation of the empirical relationship between house prices and consumption usually refers to the omission of an unob-

[9]Indeed, the point estimate becomes negative when I leave out income growth in the baseline specification.

served variable causing both house prices and consumption to change. As Attanasio et al. (2009) argue, this unobserved variable could be income expectations.

Expecting higher expected future income, forward looking households should increase today's consumption. If consumption goods and housing services enter the utility function as complements, they should also increase the demand for housing services. If housing supply is not perfectly elastic, house prices should increase endogenously.

In this paper's empirical setup, this confounder can only affect the estimate at the sub-regional level because I control for region-year fixed effects in the main regression. I use the survey waves of 1989 and 1991 to control for the individual expected income growth rate in the baseline regression that includes all fixed effects.

In 1989 and 1991 survey participants were asked to report the expectations of their incomes one year ahead. They were asked to assign probabilities to income growth rates from "more than 25%" to "0% - 3%" in 10 steps or if their income will shrink. In case they assigned a positive probability to a decrease in income, they were asked to report the decrease in percent.

I calculate the expected income growth rate as the probability-weighted average of bin mid-points. For the highest income expectation I use the lower bound of the interval of 25%, because for this option households are not asked to specify the size of the increase. If households assign a positive probability to a decreasing income, I use the reported expected decrease.

The two survey waves also include an equivalent question on inflation expectations. In Appendix 1.8.3, I provide further details on these survey questions. I use the same procedure described above to calculate an implied expected inflation rate. Then I define the expected real income growth rate as the expected income growth rate minus the expected inflation rate.[10]

[10] Figure 1.10 in Appendix 1.8.3 shows a histogram of the calculated real income growth expectations between -25% and 25%. On average, households expect a negative real income growth rate. The standard deviation of the expected real income growth rate is 6%.

Table 1.3 presents the results of the baseline model for the years 1991 and 1993 in the first column including all fixed effects. With 10.6% the elasticity estimate for those two years is somewhat larger than the one found for all survey years, but within two standard errors of the baseline estimate. The point estimate of the elasticity of real income growth is larger than that of all survey years.

The second column controls for the expected real income growth rate as calculated above. The coefficient is insignificant but the point estimate is positive, in line with the theoretical predictions. Including the expected real income growth rate does not change the estimate or the significance of the house price elasticity of consumption. Unfortunately, only two waves of the SHIW cover the questions concerning income expectations and the question only deals with next year's income growth. Nevertheless, the results do not suggest that expected income growth is simultaneously causing house price and consumption growth.

Table 1.3 suggests that, if there is a sub-regional confounding variable that affects both house price growth and consumption growth, income expectations are not a major concern for our regression specification. Since this confounder has been discussed much in the literature (Attanasio et al., 2009), the robustness of our preferred regression to adding a measure for expected income growth addresses one major concern for sub-regional endogeneity.

Why does accounting for income expectations not change the elasticity estimate conditional on region, year and region-year fixed effects? As shown in Table 1.4 individual expected income growth rates are not significantly correlated with reported house price growth rates. The reported lack of a correlation holds even unconditionally, meaning when I do not control for region, year or region-year fixed effects.

Another potential concern is that expected income growth is computed for the next year while house price growth is calculated using the last survey year's values. So lagged expected income growth may be correlated with house price growth. As the second row of Table 1.4 shows, there is no positive correlation between expected income growth in the years 1989 and 1991 and house price growth in 1991 and 1993. Figure 1.11 in Appendix 1.8.3 shows the observations used in

Table 1.4 as scatter plots together with a linear trend. The plots do not suggest any significant linear or nonlinear relationship between house price growth rates and income growth rates.

Table 1.3: *Controlling for expected income changes*

	(1)	(2)
Percentage change of real house value	0.106***	0.106***
	(0.040)	(0.040)
Percentage change of real financial assets	0.129	0.130*
	(0.079)	(0.078)
Percentage change of real income	0.453***	0.454***
	(0.062)	(0.063)
Age/100	-0.193**	-0.191**
	(0.068)	(0.069)
Change in number of family members	0.027	0.027
	(0.018)	(0.018)
Expected percentage change of real income		0.088
		(0.247)
Constant	-0.097	-0.113
	(0.080)	(0.100)
Observations	466	466
Adj. R^2	0.21	0.21
Region dummies	Yes	Yes
Year dummy	Yes	Yes
Region-year dummies	Yes	Yes

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. Robust standard errors are displayed in parentheses below the coefficients. The table reports the results using information from the survey years 1989, 1991 and 1993.

Table 1.4: *Cross-correlation of house price growth and expected income growth*

	Percentage change of real house value$_t$
Expected real income growth rate$_t$	0.049
Expected real income growth rate$_{t-2}$	-0.020

Notes: Both correlation coefficients insignificant at the 10% level. The table reports the results using information from the survey years 1989, 1991 and 1993.

1.4.3 Durable and non-durable consumption

This section deals with the response of durable and non-durable consumption to changes in house prices. Surico and Trezzi (2019) find that an increase of house prices is associated with an increase of non-durable consumption while durable consumption does not increase.

Table 1.5 reports the results for the baseline regression along the results of the baseline specification with the growth rates of non-durable and durable consumption on the left hand side.[11] Note that overall the model can explain less of the variation in durable consumption growth than non-durable consumption growth. The coefficient of real income growth is larger in the regression with durable consumption expenditures than in the regression with non-durable consumption expenditures. This finding is in line with the cyclical nature of durable consumption over the business cycle, as documented for instance by Berger and Vavra (2015) for the US.

The estimated house price elasticity of consumption is significantly different from zero only for non-durable consumption. A 1% increase of house prices is associ-

[11] Because the dependent variable is a growth rate, I exclude those observations, where durable consumption was zero in the previous observation period. Also a probit model for the extensive margin (purchase durables: yes=1, no=0) not reported for brevity does not yield a significant effect of house price changes.

Table 1.5: *Different consumption measures*

	(1) Baseline	(2) Non-durable consumption	(3) Durable consumption
Percentage change of real house value	0.052**	0.095***	0.035
	(0.020)	(0.012)	(0.220)
Percentage change of real financial assets	0.026	0.035**	-0.664
	(0.027)	(0.015)	(0.873)
Percentage change of real income	0.270***	0.118***	0.580**
	(0.037)	(0.013)	(0.230)
Age/100	-0.044	-0.052***	0.164
	(0.030)	(0.017)	(0.388)
Change in number of family members	0.062***	0.033***	0.019
	(0.010)	(0.006)	(0.104)
Became unemployed	0.029	-0.024	0.264
	(0.039)	(0.022)	(0.413)
Constant	-0.157***	0.377***	0.347
	(0.031)	(0.129)	(0.427)
Observations	10,591	10,568	2,426
Adj. R^2	0.35	0.14	0.07
Region dummies	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Region-year dummies	Yes	Yes	Yes

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variables are the annualized growth rates , $\Delta C_t/C_{t-1}$, of real consumption expenditures, real non-durable consumption expenditures and durable consumption expenditures. Robust standard errors are displayed in parentheses below the coefficients.

ated with a 0.09% increase of non-durable consumption, but only an insignificant 0.035% increase of durable consumption.

The results are in line with the fact that the estimated elasticity is in general small. In the baseline model, a 10% increase of the real house value at the mean consumption level is roughly associated with a 100€ increase of yearly consumption expenditures. The fact that the effect of housing wealth changes on durable consumption is small is in line with the empirical evidence that the wealth effect triggers few durable purchases (Surico and Trezzi, 2019).

1.5 Heterogeneity

1.5.1 The collateral effect - debt heterogeneity

Given the low but significant house price elasticity of consumption, as a next step I investigate the heterogeneity of the effect across households. Recently, a number of studies focused on households with high marginal propensities to consume close to their borrowing constraint (Kaplan and Violante, 2014). Cloyne et al. (2019) also show that equity extraction in response to house price changes increases with loan-to-value ratios. This channel is also plausible for Italy given the type of existing mortgage contracts. Theoretically, the collateral effect should be the highest for these households. Also Berger et al. (2018) find that in their theoretical model the house price elasticity of consumption increases with household leverage.

To check if it increases with household indebtedness, I calculate the "loan-to-value" ratio as:[12]

[12] I use the total value of financial liabilities, because the survey's definition of mortgage debt is not comparable over the sample period. In 2010, around 93% of total debt of homeowners was mortgage debt.

$$\text{Loan-to-value ratio}_{i,t} = \frac{\text{Financial liabilities}_{i,t}}{\text{Housing wealth}_{i,t}}. \tag{1.2}$$

Table 1.6 shows the results of the baseline specification with all fixed effects with the loan-to-value ratio added as an interaction term. The first column only adds the loan-to-value ratio and the interaction term as independent variables. The second column interacts the loan-to-value ratio also with the income growth rate and the financial asset growth rate.

Including interaction terms with the income growth rate and the financial asset growth rate changes the estimate. While the estimated interaction of the loan-to-value ratio with income growth is not significantly different from zero, it is negative and significant at the 5% level for financial asset growth. Both interaction coefficients of the house price growth rate and the loan-to-value ratio are significant at the 5% level. While the house price elasticity of consumption is at 1.6%, it increases with the loan-to-value ratio. This finding supports the collateral effect. A loan-to-value ratio of 50% for example would imply a house price elasticity of consumption of around 15%.

What does this imply for the comparison of the estimates with findings in the literature for the US? According to Beraja et al. (2019), the mean home equity share in all metropolitan statistical areas in the US at the beginning of the financial crisis was 16%, which implies a mean loan-to-value ratio of 84%. Linearly extrapolating based on the estimated specification for Italy, this would imply an estimated house price elasticity of consumption of around 24%, an estimate in the range of those from Kaplan et al. (2020a) and Christelis et al. (2015) for the US. For the sample used here, the mean loan-to-value ratio as defined above is only 6% thus implying a lower overall elasticity. The result suggests that the structure of household balance sheets in Italy that may lead to a lower overall elasticity estimate compared to the US.

This result is based on the assumption of a homogeneous interaction effect of the loan-to-value ratio on the house price elasticity of consumption. In theory, however, we should not expect such a linear effect. Households with loan-to-value ratios of 10% for instance can already borrow more, since they are far away from their borrowing constraint. Households with loan-to-value ratios of 110% will most likely be unable to borrow more, even when their house price increases, since they are already under water.

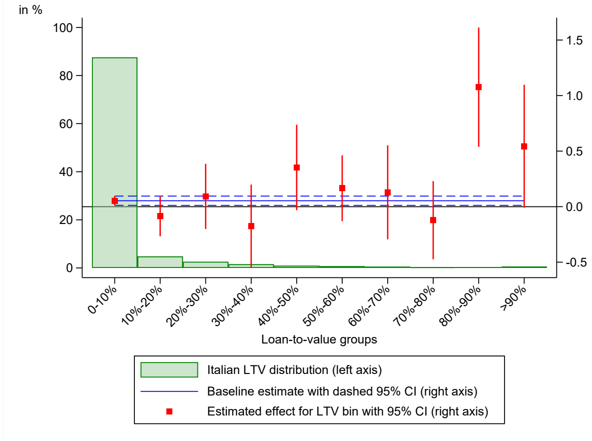
Therefore, I construct ten loan-to-value groups with 10 percentage point intervals. The first group includes households without any debt up until a loan-to-value ratio of 10%. The last group includes households with loan-to-value ratios greater than 90%. Figure 1.6 summarizes the estimated house price elasticity of consumption for all ten loan-to-value groups with the respective 95% confidence intervals. The distribution of households in the sample is shown as a bar chart. Most of Italian homeowners in the sample (88%) do either not have any debt or loan-to-value ratios below 10%.

The effect for the first loan-to-value group is significant and close to the baseline effect. Since these are only households with loan-to-value ratios below 10%, this estimate captures the wealth effect. For most of the other loan-to-value groups the elasticity estimate is not significantly different from zero, potentially due to the small group sizes. For those households with loan-to-value ratios between 80% and 90% the elasticity estimate is as large as 1. This finding supports the collateral effect, which should mostly affect households at their borrowing constraint.

Table 1.6: *Interaction with loan-to-value ratio*

	(1)	(2)
Percentage change of real house value	0.016	0.016
	(0.018)	(0.018)
Percentage change of real house value*	0.274**	0.271**
Loan to value ratio	(0.126)	(0.126)
Loan to value ratio	0.045	0.010
	(0.031)	(0.032)
Percentage change of real financial assets	0.048*	0.116***
	(0.028)	(0.038)
Percentage change of real income	0.214***	0.217***
	(0.031)	(0.033)
Age/100	-0.050	-0.054*
	(0.031)	(0.031)
Became unemployed	0.024	0.024
	(0.038)	(0.038)
Change in number of family members	0.045***	0.044***
	(0.008)	(0.008)
Percentage change of real income*		-0.057
Loan-to-value ratio		(0.102)
Percentage change of real financial assets*		-0.235**
Loan-to-value ratio		(0.117)
Constant	-0.161	-0.153
	(0.218)	(0.219)
Observations	10,367	10,367
Adj. R^2	0.11	0.12
Region dummies	Yes	Yes
Year dummies	Yes	Yes
Region-year dummies	Yes	Yes

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. Robust standard errors are displayed in parentheses below the coefficients.

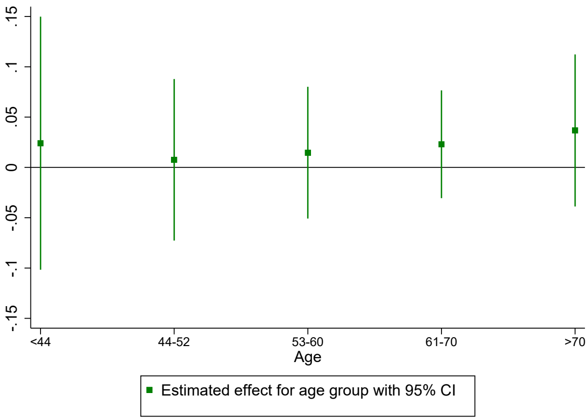


Notes: Distribution of loan-to-value ratios (left axis) for home owners over the entire sample period. The baseline effect (right axis) stems from the baseline specification, column (5) of Table 1.2, and is shown with 95% confidence intervals. The effects of house price changes across the loan-to-value distribution (right axis) are shown with 95% confidence intervals. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. The regression includes the same controls as the baseline specification, column (5) of Table 1.2. Robust standard errors.

Figure 1.6: *Loan-to-value distribution and the effect of house prices across the loan-to-value distribution*

How does this compare to US estimates? Using the loan-to-value ratios of borrowers from Beraja et al. (2019) and the share of borrowing homeowners in the US, the Italian results correspond to an aggregate homeowner elasticity of 30% for the US loan-to-value distribution. This suggests that the difference between the Italian elasticity and existing US estimates can be explained by the collateral effect, more specifically the share of households at the borrowing constraint. This finding is in line with recent estimates from the US from Guren et al. (2021), who show that the elasticity estimates should increase only at loan-to-value ratios above 0.6, because households with lower loan-to-value ratios are far enough from their borrowing constraints so that the collateral effect becomes negligible.[13]

1.5.2 The wealth effect - age heterogeneity



Notes: The effects of house price changes across age distribution are shown with 95% confidence intervals. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. The regression includes the same controls as the baseline specification, column (5) of Table 1.2. Robust standard errors. Age reported is the age of the household head. Sample of homeowners with loan-to-value ratios smaller than 10%.

Figure 1.7: *The effect of house prices for five different age groups*

Attanasio et al. (2009) find that the elasticity of consumption with respect to house prices in the UK is larger for young households which is at odds with a stronger wealth effect expected at older ages. In order to investigate the responsiveness

[13]Guren et al. (2021) also argue that the house price elasticity of consumption should fall quickly for households with loan-to-value ratios above 1, i.e., for households that have negative home equity. The small sample size inhibits further differentiation of the effects at the very top of the loan-to-value distribution in Italy.

of the elasticity estimate to age, I first split the sample at the median age of 60, which is also the effective average retirement age in Italy in 2000 according to the OECD, so in the middle of the observation period. Table 1.12 in Appendix 1.8.4 shows the estimates of the baseline regression for young and old households as a benchmark.

Attanasio et al. (2012) build a life-cycle model of consumption and housing choice and find that young households should react more strongly to income shocks than old households, while they should react less strongly to house price shocks compared to old households. Indeed, the income elasticity of consumption estimated in Table 1.12 is larger for young households. The house price elasticity estimates of the pre- and post-retirement samples are similar instead.

One reason for finding no difference in the elasticities between the groups may be that age groups differ across a number of characteristics. Table 1.13 shows the mean loan-to-value ratios over the age distribution. While the overall loan-to-value ratio for households in the sample is relatively low, it decreases with age. The wealth effect implies that old households consume more out of house price shocks. The collateral effect implies that young households with more debt consume more out of house price shocks.

Figure 1.7 shows the estimated house price elasticity of consumption for five equally sized age groups in the sample of homeowners with loan-to-value ratios smaller than 10% in order to avoid that the collateral effect confounds the findings. I find no significant difference in the house price elasticity of consumption across age groups, although the point estimate for the oldest age group (>70) is the largest. Using the whole sample of homeowners does not change the findings. Figure 1.12 in Appendix 1.8.4 shows the corresponding elasticities across age groups. A possible explanation for this finding may be the high inheritance ratio in Italy. If households expect that their main residence will be passed across generations, the timing of house price increases during their own life cycle becomes less important.

1.6 Robustness checks

1.6.1 Categorical consumption growth

In the following, I estimate an ordered probit model, where consumption growth is converted to an ordered categorical variable. This robustness check is warranted, because the growth rate of consumption, which is the dependent variable in the regressions, has a lower bound at -1. As Christelis et al. (2015) point out, the linear baseline regression model would predict consumption growth below -1 for some values of the independent variables. Furthermore, I also compare the results from the ordered probit model to those provided by Christelis et al. (2015). I construct a consumption growth variable that has three categories.

This variable states that consumption has increased if consumption growth is greater than 5%. I assume consumption to stay constant if consumption growth is between -5% and 5%.[14] The categorical variable states that consumption has decreased if consumption growth is below -5%.

Table 1.7 provides the marginal effects at the means of the independent variables on the probabilities that consumption decreases, stays the same, or increases. A positive growth rate of real financial assets significantly increases the probability that the household increases consumption. A growth in real income significantly increases the probability that the household increases consumption. All marginal effects of the remaining control variables have the sign expected given the results from the baseline specification.

The effect of house price growth on the probability that consumption increases is positive but not significant. The sign is in line with the results from the baseline specification. I calculate the predicted probabilities that consumption increases for a 15% increase of house prices to quantify the estimated effect and to compare it to the results from Christelis et al. (2015). I find that a 15% increase of house

[14] There are no households in the sample with a consumption growth rate that exactly equals zero. Table 1.14 in Appendix 1.8.5 shows the results of a regular probit regression with two categories of consumption growth (increase and decrease).

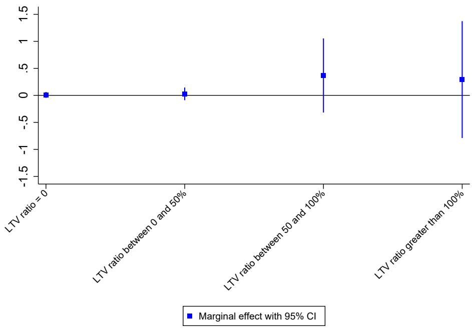
Table 1.7: *Ordered probit results*

	Marginal effect	SE
Percentage change of real house value		
Probability that real consumption decreases	-0.034	(0.025)
Probability that real consumption stays the same	-0.002	(0.001)
Probability that real consumption increases	0.035	(0.026)
Percentage change of real financial assets		
Probability that real consumption decreases	-0.072*	(0.042)
Probability that real consumption stays the same	-0.004*	(0.002)
Probability that real consumption increases	0.076*	(0.044)
Percentage change of real income		
Probability that real consumption decreases	-0.327***	(0.076)
Probability that real consumption stays the same	-0.017***	(0.005)
Probability that real consumption increases	0.345***	(0.081)
Age/100		
Probability that real consumption decreases	0.015	(0.032)
Probability that real consumption stays the same	0.001	(0.002)
Probability that real consumption increases	-0.016	(0.034)
Change in number of family members		
Probability that real consumption decreases	-0.092***	(0.016)
Probability that real consumption stays the same	-0.005***	(0.001)
Probability that real consumption increases	0.097***	(0.017)
Observations	10,591	

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the marginal effects at means from the ordered probit specification. Standard errors are displayed in parantheses in the right column. The dependent variable is the categorical change in consumption. Consumption is assumed to stay the same if the growth rate of real consumption is above -5% and below 5%. Rounded baseline probabilities: Probability that real consumption decreases 37.4% , probability that real consumption stays the same 13.3% , probability that real consumption increases 49.3%.

prices increases the probability of an increase in consumption by 0.5 percentage points, which is one third of the increase that Christelis et al. (2015) find. Thus, the results from the ordered probit model underline the previous finding that the association of house prices and consumption is smaller than in most studies that use US data. Table 1.14 provides the results of a probit model with only two consumption categories which yields similar estimates.

We can also use the ordered probit model to estimate the effect of house prices on the probability of a consumption increase for indebted households. Figure 1.8 summarizes the marginal effects of a percentage change in real house prices at the means for an ordered probit model for four groups of households: households with no debt, those with loan-to-value ratios between 0 and 50%, 50% and 100% and above 100%. The point estimates of the marginal effects of an increase in house prices is higher for more indebted households, in line with the results from the baseline OLS regression.



Notes: The figure shows the marginal effects at means on the probability that real consumption increases of house price changes for four separate ordered probit regressions with 95% confidence intervals. The dependent variable in these regressions is the categorical change in consumption.

Figure 1.8: *Debt heterogeneity of ordered probit results*

Overall, using categorical consumption growth as the dependent variable somewhat reduces the significance of the effect of house prices on consumption. Nevertheless, the sign and size of the estimated effect are in line with the baseline findings. Further, the results add to evidence that the effect of house prices on consumption is larger for more indebted households.

1.6.2 Restricted sample

One potential objection could be that there are very large observations for both consumption and house prices growth. Therefore, I exclude all annualized consumption and house price growth rates below -50% and above 50%. Table 1.8 reproduces the estimates from the baseline regression using only consumption and house price growth rates within the given range.

Table 1.8: *Restricted sample*

	(1)	(2)	(3)	(4)	(5)
Percentage change of real house value	0.028	0.028	0.027	0.030	0.032*
	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)
Percentage change of real financial assets	0.040**	0.050***	0.051***	0.055***	0.052***
	(0.019)	(0.019)	(0.019)	(0.020)	(0.018)
Percentage change of real income	0.124***	0.107***	0.107***	0.106***	0.130***
	(0.029)	(0.028)	(0.028)	(0.028)	(0.022)
Age/100		-0.024	-0.026	-0.031	-0.026
		(0.022)	(0.022)	(0.022)	(0.020)
Change in number of family members		0.041***	0.042***	0.042***	0.043***
		(0.007)	(0.006)	(0.007)	(0.007)
Became unemployed		0.004	0.003	0.006	0.011
		(0.029)	(0.028)	(0.030)	(0.030)
Constant	-0.003	0.014	0.018	0.026	-0.094***
	(0.003)	(0.013)	(0.018)	(0.021)	(0.018)
Observations	9,779	9,779	9,779	9,779	9,779
Adj. R^2	0.03	0.03	0.04	0.06	0.10
Region dummies	No	No	Yes	Yes	Yes
Year dummies	No	No	No	Yes	Yes
Region-year dummies	No	No	No	No	Yes

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. Robust standard errors are displayed in parentheses below the coefficients.

The estimated house price elasticity of consumption is lower than the baseline estimate and insignificant in the first four columns. The baseline specification (column 5) yields a lower point estimate of the elasticity, which is significant at the

10% level. The regression coefficient of the percentage change of real income also becomes smaller. Overall, however, the elasticity estimate is robust to excluding very small and large values for consumption and house price growth. In general, the lower adjusted R^2 shows that the model is able to explain less of the variation in consumption growth than the baseline model.

1.7 Conclusion

In this paper, I estimate the house price elasticity of consumption using individual household data from Italy. The data set consists of households that are considerably less indebted than households in the US. Due to the data set structure I can use the individual variation in house price growth to identify the effect on consumption growth. Therefore, I do not need to rely on regional variation in house prices which reduces the scope for omitted variable bias. Further, the long time horizon of the data set allows to control for macroeconomic conditions with year fixed effects and regional business cycles with region-year fixed effects. In particular, the latter turns out to be important for the size of the elasticity estimate.

I construct individual house price growth by using only households that live in the same housing units. I find that while households are only asked about their perception of their house value, the median growth rates of house prices by year closely tracks two independent house price indices which makes the difference across house price growth rates, based on self-reported valuations of housing units, a plausible measure to infer the house price elasticity of consumption.

The estimated elasticity in the baseline model is around 5% so that a 10% increase in house prices is associated with a 0.5% increase in consumption expenditures. The effect is robust to including changes in income and financial wealth as well as to including household characteristics. Additionally, the effect is lower when controlling for years, region and region-year fixed effects. Therefore, the estimates reported in this paper improve upon existing estimates that either rely on regional variation to identify the effect or only include certain time periods, which makes it difficult to control for macroeconomic conditions that could affect both house prices and consumption.

Using a question on income expectations from two waves of the SHIW, I do not find evidence for real income expectations being the common cause of consumption and house price growth, as has been suggested in the literature. This is due to the fact that, in the sample, the correlation between households' income expectations and house price growth is close to zero.

With respect to the collateral effect, I find that the estimated house price elasticity of consumption increases with household indebtedness relative to the housing value. This is in line with the theoretical literature stressing the collateral effect, which implies that house price increases make the collateral constraint less binding. This effect is more relevant for more indebted households. I find that this effect can explain the difference in the size of the estimated elasticity compared to the findings from the US. Using a linear interaction effect, and assuming that Italian households are as indebted US households before the Great Recession, the estimates imply that the aggregate elasticity of consumption with respect to house price changes would increase to a value of 0.24. A more detailed heterogeneity analysis with ten different loan-to-value groups shows that the highest elasticity estimate is estimated for households at their borrowing constraint. Extrapolating these results using the US pre-crisis loan-to-value distribution yields an aggregate elasticity for homeowners of around 30%, close to what studies find for the US.

With respect to the wealth effect, I find no significant differences in elasticity estimates across age groups, a finding add odds with predictions from the theoretical literature. This result may be explained by the high ratio of inherited houses in Italy, which makes the timing of house price increases relative to overall family tenure less important.

I also estimate an ordered probit model with consumption growth as a categorical variable. I find that a 15% increase in house prices increases the probability that a household increases consumption by 0.5 percentage points. For households with a loan-to-value ratio of 50% the same increase of house prices is associated with a 4 percentage point increase in the probability of an increase of consumption.

The results show that there is substantial heterogeneity in house price elasiticities of consumption between countries. They can inform policy makers about potential regional responses to large house price changes. Further, they can provide useful targets for future research using structural models.

1.8 Appendix of chapter 1

1.8.1 Additional descriptive statistics

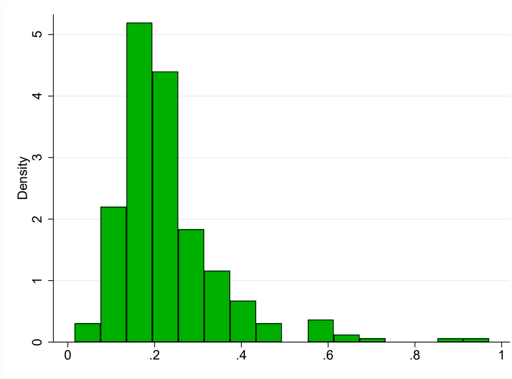
Table 1.9 documents the regional heterogeneity in economic activity in Italy.

Table 1.9: *Gross domestic product (GDP) in euro per inhabitant in 2017*

	Region	GDP per capita
1	Lombardia	38,000
2	Provincia Autonoma di Trento	36,100
3	Emilia-Romagna	35,300
4	Valle d'Aosta	35,200
5	Veneto	33,100
6	Lazio	32,700
7	Liguria	31,600
8	Friuli-Venezia Giulia	30,900
9	Toscana	30,400
10	Piemonte	30,300
11	Marche	26,800
12	Abruzzo	24,700
13	Umbria	24,500
14	Basilicata	21,100
15	Sardegna	20,600
16	Molise	19,800
17	Puglia	18,400
18	Campania	18,200
19	Sicilia	17,500
20	Calabria	17,200

Sources: Eurostat (Gross domestic product (GDP) at current market prices by NUTS 2 regions, code *nama_10r_2gdp*).

Figure 1.9 shows the standard deviations of house price growth rates in region-years. It documents that there is substantial variation of house prices even if I control for region and year fixed effects.



Notes: Histogram of standard deviations of house price growth rates in region-years.

Figure 1.9: *Histogram of standard deviations of house price growth in regions in survey years.*

1.8.2 Robustness of main specification

Table 1.10 shows the results of the baseline specification with standard errors clustered at the regional level.

Table 1.10: *Regional standard error clustering*

	(1)	(2)	(3)	(4)	(5)
Percentage change of real house value	0.096*	0.094*	0.095*	0.081**	0.052*
	(0.050)	(0.047)	(0.048)	(0.034)	(0.027)
Percentage change of real financial assets	0.015	0.035**	0.033**	0.030	0.026
	(0.013)	(0.014)	(0.015)	(0.019)	(0.018)
Percentage change of real income	0.392***	0.354***	0.353***	0.300***	0.270***
	(0.125)	(0.110)	(0.110)	(0.078)	(0.047)
Age/100		-0.039	-0.036	-0.048*	-0.048
		(0.025)	(0.027)	(0.026)	(0.029)
Change in number of family members		0.091***	0.091***	0.078***	0.062***
		(0.026)	(0.026)	(0.017)	(0.013)
Became unemployed		0.025	0.028	0.027	0.029
		(0.025)	(0.026)	(0.026)	(0.030)
Constant	0.021***	0.049***	0.051**	0.073	-0.157***
	(0.005)	(0.016)	(0.019)	(0.046)	(0.038)
Observations	10,591	10,591	10,591	10,591	10,591
Adj. R^2	0.18	0.19	0.20	0.21	0.35
Region dummies	No	No	Yes	Yes	Yes
Year dummies	No	No	No	Yes	Yes
Region-year dummies	No	No	No	No	Yes

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. Standard errors clustered at the region shown in parentheses below the coefficients.

1.8.3 Income expectations

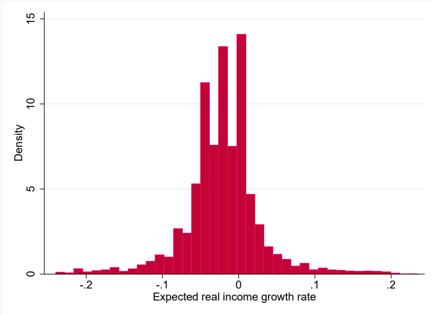
Table 1.11 shows the options households can assign probabilities to in the survey waves of 1989 and 1991.

Table 1.11: *Income / inflation expectation options in the SHIW*

Growth rate option in SHIW	Number used for calculation of expectation
More than 25%	25%
Between 20 and 25%	22.5%
Between 15 and 20%	17.5%
Between 13 and 15%	14%
Between 10 and 13%	11.5%
Between 8 and 10%	9%
Between 7 and 8%	7.5%
Between 6 and 7%	6.5%
Between 5 and 6%	5.5%
Between 3 and 5%	4%
Between 0 and 3%	1.5%

Notes: The table shows the options households can assign probabilities to when they are asked for their nominal income and inflation expectations in the upcoming year. The right column gives the growth rate used for calculating the weighted average. The table reports the results using information from the survey years 1989 and 1991.

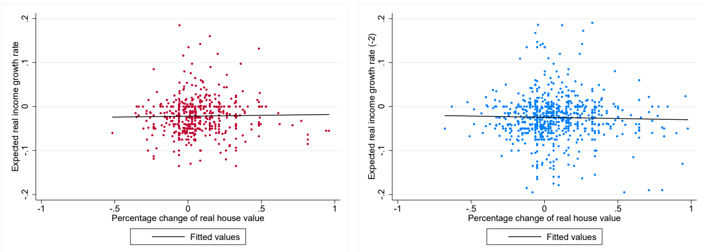
Figure 1.10 shows the dirstribution of real income expectations in the survey waves of 1989 and 1991.



Notes: The figure shows the distribution of expected real income growth rates that are calculated as the difference between the weighted averages of expected income expectations and inflation expectations.

Figure 1.10: *Histogram of calculated expected income growth rates*

Figure 1.11 documents that there is no obvious correlation between house price growth rates and expected real income growth rates.



Notes: The left panel shows the correlation between house price growth rates in the survey waves of 1989 and 1991 and expected real income growth rates in 1989 and 1991. The right panel shows the correlation between house price growth rates in 1991 and 1993 and expected real income growth rates in 1989 and 1991.

Figure 1.11: *Unconditional association of house price growth and expected income growth*

1.8.4 Age heterogeneity

Table 1.12 shows the results of the baseline specification estimated separately for age groups split at the effective retirement age.

Table 1.12: *Age group sample split*

	(1)	(2)
Age of household head	≤ 59	> 59
Percentage change of real house value	0.054*	0.058**
	(0.031)	(0.024)
Percentage change of real financial assets	0.015	0.057
	(0.030)	(0.043)
Percentage change of real income	0.311***	0.190***
	(0.044)	(0.051)
Age/100	0.061	0.049
	(0.075)	(0.060)
Became unemployed	0.027	0.072
	(0.042)	(0.047)
Change in number of family members	0.076***	0.050***
	(0.016)	(0.012)
Constant	0.249	-0.168***
	(0.372)	(0.049)
Observations	5,292	5,299
Adj. R^2	0.44	0.17
Region dummies	Yes	Yes
Year dummies	Yes	Yes
Region-year dummies	Yes	Yes

Notes: Sample split at effective retirement age. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t / C_{t-1}$. Robust standard errors are displayed in parentheses below the coefficients.

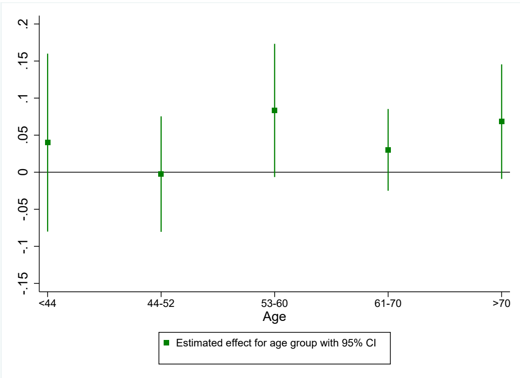
Table 1.13 shows the mean ltv ratio for four age groups. It documents that ltv ratios decrease with age.

Table 1.13: *Age and loan-to-value ratios*

	Mean loan-to-value ratio	Observations
Age < 50	0.12	2,650
$50 \leq$ Age < 60	0.06	2,498
$60 \leq$ Age < 70	0.03	2,541
Age ≥ 70	0.01	2,678

Notes: Mean loan-to-value ratios across age groups. Mean loan-to-value ratio for all age groups at 0.06.

Figure 1.12 shows the consumption elasticities of house prices across the age distribution for the whole sample of homeowners.



Notes: The effects of house price changes across age distribution are shown with 95% confidence intervals. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. The regression includes the same controls as the baseline specification, column (5) of Table 1.2. Robust standard errors. Age reported is the age of the household head. Sample of all homeowners.

Figure 1.12: *The effect of house prices for five different age groups*

1.8.5 Further robustness checks

Table 1.14 shows the marginal effects for the probit specification.

Table 1.14: *Probit regression*

	Marginal effect SE
Percentage change of real house value	0.024
	(0.029)
Percentage change of real financial assets	0.055
	(0.046)
Percentage change of real income	0.357***
	(0.073)
Age/100	-0.074
	(0.053)
Change in number of family members	0.086***
	(0.017)
Observations	10,605

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports the marginal effects at means from the probit specification. Standard errors are displayed in parantheses below the marginal effects. The dependent variable is the categorical change in consumption (Increase of real consumption, YES=1).

The results of the baseline specification for the sample of renters is shown in Table 1.15.

Table 1.15: *Renter regression*

	(1)	(2)	(3)	(4)	(5)
Percentage change of real house value	0.025	0.026	0.027	0.025	0.023
	(0.025)	(0.025)	(0.026)	(0.025)	(0.026)
Percentage change of real financial assets	0.061	0.057	0.076	0.079	0.073
	(0.048)	(0.051)	(0.054)	(0.052)	(0.053)
Percentage change of real income	0.587***	0.584***	0.584***	0.583***	0.567***
	(0.055)	(0.057)	(0.057)	(0.057)	(0.059)
Age/100		0.015	0.022	0.018	0.032
		(0.042)	(0.041)	(0.041)	(0.043)
Change in number of family members		0.023	0.019	0.023	0.023
		(0.018)	(0.018)	(0.018)	(0.017)
Became unemployed		-0.003	0.001	0.001	0.015
		(0.039)	(0.039)	(0.038)	(0.041)
Constant	0.012*	0.005	-0.001	0.035	0.055*
	(0.007)	(0.024)	(0.053)	(0.055)	(0.033)
Observations	3,029	3,029	3,029	3,029	3,029
Adj. R^2	0.69	0.69	0.70	0.70	0.73
Region dummies	No	No	Yes	Yes	Yes
Year dummies	No	No	No	Yes	Yes
Region-year dummies	No	No	No	No	Yes

Notes: Regression for renters. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the annualized growth rate of real consumption expenditures, $\Delta C_t/C_{t-1}$. Robust standard errors are displayed in parentheses below the coefficients.

2 On the Transmission of Monetary Policy to the Housing Market

This chapter has been prepared together with Winfried Koeniger from the University of St. Gallen and Marc-Antoine Ramelet from the Swiss National Bank.

Abstract:
We provide empirical evidence on the heterogeneous transmission of monetary policy to the housing market across and within countries. We use household-level data from Germany, Italy and Switzerland together with the respective monetary policy shocks identified from high-frequency data. We find that the pass-through of monetary policy shocks to rates of newly originated (fixed-rate) mortgages is twice as strong in Switzerland than in Germany and Italy. After an accommodative monetary policy shock, this is associated in the housing market with a larger immediate, and persistent increase of transitions from renting to owning; a stronger decrease in rents; and an increase of the price-rent ratio. Within Italy, we find a stronger pass-through to mortgage rates, housing tenure transitions and the price-rent ratio in the northern regions that have been characterized in the literature as more financially developed than the southern regions.

JEL Codes: E21, E52, R21
Keywords: Monetary policy transmission, Housing market, Home ownership, Rents, House prices

2.1 Introduction

The transmission of monetary policy is at the core of the research agenda in economics. Much research has focused on the response of consumption and output to shocks to the policy rate (Galí, 2015). Recent research by Calza et al. (2013) and Corsetti et al. (2021) has documented a sizable heterogeneity of monetary policy transmission across euro area countries, and that this heterogeneity is associated with differences in the housing market.

We contribute to that literature by providing evidence at the household level on the transmission of monetary policy to the housing market. We focus on Germany, Italy and Switzerland, which differ in at least two important dimensions: (i) the size of the market for rental housing and its ownership structure and (ii) the indebtedness of new homeowners and the characteristics of the mortgage market. We explain in Section 2.2 that these dimensions matter for the transmission of policy rate shocks to the homeownership rate and the price-rent ratio because they affect the pass-through to the rental price of housing units and the user cost of owning a home.

We estimate the transmission to the housing market using household-level data together with monetary policy shocks identified from high-frequency data. Our use of household-level data has the advantage that we can analyze transitions (gross flows) across housing tenure states of individual households, together with the pass-through of the policy rate shocks to rents and housing values. Analyzing the differences in the pass-through across households yields insights on the causes for the heterogeneous transmissions across countries.

We find that the pass-through of an unexpected change of the policy rate to rates of newly originated (fixed-rate) mortgage rates is about 80% in Switzerland but only half that in Germany and Italy. After an unexpected reduction in the policy rate by 25 bp, transitions from renting to owning a home increased by $1 - 2$ pp in Germany and Switzerland but not in Italy, whereas transitions from owning to renting increased by 0.5 pp in Switzerland but not significantly in the other countries. These effects on the gross flows for Germany and Switzerland are quantitatively important, as illustrated by considering a policy rate shock of one standard deviation. Then the effects on the transitions previously mentioned must be scaled down by approximately one third because the standard deviation of the monetary policy shocks is 7 bp for the European Central Bank (ECB) and 10 bp for the Swiss National Bank (SNB). The resulting effects on the transitions remain sizable given that the average rate per year, at which households change housing tenure from renting to owning for the considered countries, is 4 percent and the average rate per year, at which households change from owning to renting, is $1-2$ percent.

The implied increase in the net flow toward owning after a policy rate reduction in Germany and Switzerland is associated with a stronger increase of the price-rent ratio in Switzerland than in Germany and Italy. Rents decrease by 3.5 percent in Switzerland but we do not detect significant decreases in rents for the other two countries. We provide suggestive evidence that public ownership of rental housing, which is less important in Switzerland than in Germany and Italy, together with the indexation of rents to mortgage interest rates in Switzerland, as described further in Section 2.2, may explain the different response of rents across countries.

We uncover the regional heterogeneity of the pass-through to the mortgage rate *within* Italy, which is associated with differences in financial development. We find that an unexpected interest rate reduction triggers more transitions to homeownership and a stronger decrease of rents in more financially developed Italian regions. From a methodological point of view, the regional heterogeneity within Italy allows for an alternative identification of monetary policy transmission to the housing market. Both the results across countries and across regions within Italy illustrate how differences in the pass-through to mortgage rates are associated with differences in the transmission to housing tenure transitions, rents and price-rent ratios.

These results are of interest because monetary policy transmission to quantities and prices in the housing market matters not only for the housing market itself but also for the response of aggregate non-housing consumption. The implied distributional effects across renters and mortgagors, for example, affect aggregate consumption because these subgroups of the population differ in their marginal propensity to consume (Cloyne et al., 2020). Hence, from an applied theoretical perspective, our results provide targets for the considered countries that help to discipline quantitative models with housing which attempt to capture these distributional effects, along the lines of recent research, e.g., by Kaplan et al. (2020b), Hedlund et al. (2016), Wong (2021) for the U.S., or Kaas et al. (2021), Hintermaier and Koeniger (2018) for countries in the euro area.

Monetary policy transmission to rental prices in the housing market also matters for changes of the consumer price index, a key target of central banks. Indeed, Dias and Duarte (2019) show for the U.S. that the consumer price responses to monetary policy shocks are much stronger if the price for shelter is excluded because rents *decrease* after an (expansionary) unexpected reduction in the policy rate. Our analysis suggests that this effect is particularly relevant for Switzerland where rents decrease strongly after policy rate reductions and the incidence of renting is high, and less so for Germany where public ownership of rental units seems to mitigate the pass-through of monetary policy shocks to rents. The latter also applies to Italy where, in addition, the incidence of renting is much lower than in Germany and Switzerland (see Section 2.2).

The empirical literature on the transmission of monetary policy to the housing market is small compared with the vast literature on consumption responses (Piazzesi and Schneider, 2016). Beraja et al. (2019) and Wong (2021) focus on the mortgage refinancing channel for consumption responses which is important for the U.S. where refinancing is not as costly as in the countries we analyze (see Section 2.2). We refer to Cloyne et al. (2020) for a concise overview of the recent literature. Cloyne et al. (2020) estimate heterogeneous consumption responses across housing tenure groups and show how these responses relate to the different balance sheet positions of these groups.[1] They do not find an economically and statistically significant effect of monetary policy shocks on housing tenure shares in the U.S. and U.K. (see their online appendix). Fuster and Zafar (2021) find small effects of changes in financing costs on the willingness to pay for house pur-

[1] Slacalek et al. (2020) gauge the importance of balance sheet effects in the euro area. Collateral constraints, as emphasized by Iacoviello (2005) for example, imply that the response of house prices to expansionary monetary policy shocks may amplify the consumption response.

chases, based on a strategic survey in which respondents in the U.S. revealed their behavioral responses to hypothetical changes. Using high frequency identification of monetary policy shocks for the U.S., Dias and Duarte (2019) find instead that the homeownership rate and house prices decrease whereas rents increase after a contractionary policy rate shock.

Given the differences of housing markets across countries, the external validity of the U.S. evidence is limited. The aggregate evidence for the euro area by Corsetti et al. (2021) shows that important differences exist in the monetary policy transmission to the housing market across countries and that this heterogeneity matters for consumption responses. Our focus on three European countries allows us to analyze in greater detail the transmission to the housing market because we can provide disaggregate evidence on household transitions (gross flows) across housing tenure states and the response of rents and, for Germany and Italy, also house prices at the household level. Household-level data allow us to uncover heterogeneous effects on housing tenure transitions across population groups with different ages, incomes and net worth which provide useful targets for structural models of the housing market.

We have motivated the choice of countries for the analysis mentioning key differences in housing markets across these countries. Switzerland, which participates in the single European market with a monetary policy independent of the euro area, provides for an interesting comparison with Germany and Italy. Considering Italian- and German-speaking households within Swiss regions, allows us to assess the behavioral differences in that comparison that may be associated with culture, and that have been found to be relevant in research on household finances and housing (Haliassos et al., 2017). We find little evidence for different responses of housing tenure transitions to monetary policy shocks across language groups in Switzerland. This lack of evidence suggests that the cross-country differences in the monetary policy transmission to the housing market, which we report in this chapter, are the result of institutional differences across regions, such as the practice of benchmarking rents to the mortgage rate in Switzerland, rather than culture.

We find that the responses of the homeownership rate, rents and house prices differ across regions with a different ownership structure of housing units. These results relate to the argument of Greenwald and Guren (2019) who show that the response of the homeownership rate to changes in credit conditions should be relatively stronger than the change in the price-rent ratio in regions with less segmented housing markets, i.e., in regions where more of the housing stock is owned by large deep-pocket investors. In our analysis, an unexpected reduction of the interest rate reduces the cost of financing homes and thus improves credit conditions for households. In Section 2.4 we show in detail how monetary policy shocks pass through to yields of bonds with different maturities and to mortgage rates in each of the considered countries.

We identify monetary policy shocks using high frequency data. This approach, pioneered by Cook and Hahn (1989), Cochrane and Piazzesi (2002) and Kuttner (2001), exploits the fact that data on futures or swap contracts contain information on market expectations about monetary policy. The identification of monetary policy shocks then uses the discontinuous changes in these expectations in a short time window around the monetary policy announcements. Recent applications of this approach are in Gertler and Karadi (2015) and Nakamura and Steinsson (2018a) for the U.S., Gerko and Rey (2017) and Cesa-Bianchi et al. (2020) for the U.K., Altavilla et al. (2019) and Corsetti et al. (2021) for the euro area, and Ranaldo and Rossi (2010) for Switzerland.

Our analysis proceeds in the following steps. In Section 2.2 we briefly describe important features of the housing and mortgage markets in Germany, Italy and Switzerland, and we explain why these features matter for monetary policy transmission. We then discuss in Section 2.3 how we identify exogenous policy rate movements. In Section 2.4, we analyze the pass-through of the monetary policy shocks to long-term interest rates, and in particular to mortgage rates. We then present the household-level data for Germany, Italy and Switzerland in Section 2.5. In Sections 2.6 and 2.7, we estimate the responses of housing tenure, rents and the value of housing. In Section 2.8, we provide results for these responses across Italian regions before concluding in Section 2.9.

2.2 Housing markets and monetary policy transmission

Household portfolios, particularly homeownership rates and household debt, differ widely across countries (see, for example, Christelis et al., 2013). Table 2.1 illustrates this for Germany, Italy and Switzerland, in terms of the incidence of mortgage debt, the indebtedness of households, the size of the rental market and the ownership structure of housing units. After further describing these differences in the housing market, we discuss their relevance for the transmission of monetary policy.

Column 1 of Table 2.1 shows that less than half of the German and Swiss households own the home in which they live, implying the lowest owner occupation rates in the OECD. In contrast, in Italy the size of the rental market is much smaller given an owner occupation rate of more than three quarters.[2] The rental market does not only differ in size across the considered countries but also in terms of its ownership structure. Column 2 of Table 2.1 shows that large real estate investors, i.e., private firms and pension funds, hold almost 40% of the rental housing stock in Switzerland, 10% in Germany and less than 5% in Italy. Publicly owned housing accounts for one third of the rental housing stock in Germany, one fifth in Italy and only one tenth in Switzerland.[3] Columns 3 and 4 of Table 2.1 illustrate that the incidence and size of household debt also differ widely across the considered countries, and are largest in Switzerland and smallest in Italy.

The extent of household leverage, homeownership and the ownership structure of rental housing matters for the transmission of monetary policy to the housing market in terms of housing tenure choices, rents or house prices.[4] After a shock

[2] Thus, the owner occupation rate in Italy is larger than those in the U.S. or the U.K. where about two thirds of households own their first residence. Table 2.1 displays the owner occupation in the year 2014. During 2000-2014, the owner occupation rate has increased between 3 and 4 percentage points in Germany, Italy and Switzerland.

[3] Moreover, private households own three quarters of the rental housing stock in Italy compared with approximately 50% in Germany and Switzerland. See the notes to Table 2.1 for the data sources.

[4] A related body of the literature analyzes how the illiquidity of assets, such as housing, matters for the monetary policy transmission to both nondurable and durable consump-

to the policy rate, households revise their decision to consume housing services by renting or owning the accommodation in which they live. Whether households change their housing tenure after the shock depends on the user cost of owning a home relative to the rental price for housing services. Diaz and Luengo-Prado (2008) show in a life-cycle model with illiquid housing that a change in the mortgage interest rate has a stronger effect on the user cost of owning a house if households expect to be more leveraged when owning the home. We aim to estimate empirically the price and quantity responses in the housing market to demand shocks for owned housing that have been triggered by changes to this user cost resulting from monetary policy.

Table 2.1: *Heterogeneity in housing markets*

	Owner occupation rate	Rental housing owned by private firms (%)	Incidence of mortgage debt	Household debt per GDP
Germany	46	10	49	63
Italy	79	4	15	49
Switzerland	38	38	78	114

Sources: Owner occupation rate: ECB (Statistical Data Warehouse, Dataset *SHI*, Key *SHI.A.DE.TOOT.P*), SHIW, SFO (Federal Population Census, Table *09.03.02.01.01*). Housing ownership: SOEP, SHIW, FSO (ownership type for rental housing, Table *09.03.03.50*). Incidence of mortgage debt: SOEP, SHIW, SHP. Household debt: IMF (Global Debt Database, Private debt, Household debt, all instruments). *Notes:* The first column shows owner occupation rates in 2014 in percent. The second column shows the ownership of rented housing by private firms and pension funds in 2016 in Germany (SOEP), in 2016 in Italy (SHIW), and 2017 in Switzerland (SFO), given data availability. The third column shows the percentage of homeowners with mortgage debt in 2016 for Germany, Italy and Switzerland. The fourth column displays average household debt over GDP during 2000-2016.

The degree to which house prices and rents and thus the price-rent ratio respond to monetary policy shocks also depends on the ownership structure of rental housing.[5] If rental units are owned by deep-pocket private investors, then housing markets are less segmented such that the supply of these rental units to households willing to buy is more elastic (Greenwald and Guren, 2019). The more segmented

tion. Without sufficient liquidity in the asset portfolio, the marginal propensity to consume out of transitory income shocks increases (Kaplan and Violante, 2014), which is a key determinant of the consumption response to interest rate changes (Auclert, 2019).

[5] Over the time horizon for which we measure the effects of the monetary policy shocks, housing construction has a negligible effect on housing supply. Hence, the elasticity of the housing supply over that horizon is mostly determined by the ownership structure of existing housing units.

housing markets are instead, the stronger is the response of the price-rent ratio relative to the quantity response after a demand shock for owned housing, triggered by an unexpected monetary policy shock that has passed through to the user cost of owning a home.

Thus, public ownership of rental units may affect the transmission of monetary policy to the housing market. Publicly owned rental housing that is not for sale reduces the supply of housing units that potential homeowners can buy. Furthermore, rents of publicly owned units may react less to changes of market interest rates in Germany and Italy than in Switzerland where rents are indexed to a reference mortgage rate.[6]

Given the previous discussion, a key part of the monetary policy transmission to the housing market is the pass-through of monetary policy shocks to mortgage rates. This pass-through is particularly relevant for new mortgagors who are purchasing a home. For existing mortgagors, shocks to the policy rate have a stronger effect on cash flows if they have an adjustable-rate mortgage, can refinance a fixed-rate mortgage or release home equity at a low cost (Calza et al., 2013).

The incidence of mortgage types differs considerably across countries (see Badarinza et al., 2018, ECB, 2009, and the references therein). Typical mortgage contracts in Germany, Italy and Switzerland have different characteristics relative to those in the U.S. and the U.K., which have been analyzed in most of the literature. Most households in the U.K. have adjustable rate mortgage contracts and they can release home equity. In the U.S. most households have fixed-rate mortgages but can refinance their mortgages at little cost (ex post, the bank bears the cost of foregone interest if a household decides to refinance). Therefore, a decrease in the mortgage interest rate reduces the mortgage payments of existing indebted homeowners more in the U.K. and the U.S. than in Germany and Switzerland, where most mortgage contracts have a fixed rate, refinancing is very costly and possibilities for equity release are not common. Italy is an intermediate case because

[6] Until 2008, the reference rate was the average mortgage rate recorded by banks at the cantonal level. Since then, there has been a single national reference average rate. Whether rents are indeed adjusted after a change in the mortgage interest rate depends on whether landlords and tenants agree to implement the change.

mortgage contracts with fixed or adjustable rate are equally prevalent and costs to refinance mortgages have decreased since 2007, when pre-payment penalities were banned.[7]

Table 2.39 in the data appendix 2.10.5 shows that the incidence and type of mortgages also differ across Italian regions with different degrees of financial development, whereas the homeownership rate is similar. The incidence of mortgagors, as percentage of owners, is 3.3 percentage points higher in financially developed regions and is 7 percentage points higher if we consider new owners. Furthermore, the incidence of flexible-rate mortgages among mortgagors is 16.6 percentage points higher in financially more developed Italian regions.

Whether it is attractive to become a *new* homeowner depends on whether the pass-through of policy rate shocks decreases the user cost of owning relative to renting housing services. As we show in Section 2.4, the pass-through of the policy rate shocks to rates of newly originated fixed-rate mortgages implies persistent effects of monetary policy shocks in Germany, Italy and Switzerland. This persistence is qualitatively similar to that in the U.S. (Nakamura and Steinsson, 2018a) and the U.K. (Gerko and Rey, 2017).

2.3 Identification of monetary policy shocks

For Germany and Italy monetary policy is decided by the European Central Bank (ECB). The three key interest rates set by the ECB are, in increasing order of the value of the rates, the rate on the deposit facility, the rate on the main refinancing

[7] The percentage of variable-rate mortgages as percentage of new loans is 15% in Germany compared with 47% in Italy (ECB, 2009, Table 2). For Switzerland, Basten and Koch (2015) provide evidence using 12,700 representative mortgage transactions between 2008 and 2013 from the online platform *Comparis*. They show that contracts with rates that are fixed for four years or more accounted for around 75% of all contracts in Switzerland, where contracts with rates that are fixed for ten years accounted for 35% of new contracts and contracts with rates that are fixed for five years accounted for 26%. Only 5% of new mortgage contracts had an adjustable rate. Basten and Koch (2015) further show that changes in house prices mostly affect mortgage volumes through new mortgagors rather than through refinancing activities of existing mortgagors.

operations and the rate on the marginal lending facility. For Switzerland, the target rate was the three-month Swiss-Franc Libor during the period we consider, together with the range set by the Swiss National Bank (SNB). We construct a time series of monetary policy shocks for the period 2000 − 2017, given the availability of the other data used in our analysis, the introduction of the euro and the targeting of the three-month Swiss-Franc Libor by the SNB during 2000-2019. Because the policy rates of the ECB and SNB co-move with economic conditions,[8] we need to construct a measure of exogenous changes in the interest rate for the empirical analysis.

We identify monetary policy shocks using high-frequency data on the changes in financial-market expectations, which are contained in futures contract prices on interest rates in narrow time windows around the dates of monetary policy announcements. The identification of monetary policy shocks relies on the assumption that changes in the price of futures in these narrow time windows are the result of news contained in the policy announcements and not the result of other events that are systematically related to the monetary policy shocks. For our benchmark estimates we use time windows of one day, between the end of the announcement day and the day before, and we check the robustness for narrower time windows.

As mentioned in the analysis of Wong (2021) for the U.S., one concern may be that policymakers have private information about the state of the economy which is correlated with economic outcomes and thus household decisions. In this case, the measured policy shock consists of the true shock and an error which may be correlated with housing tenure or other economic outcomes. Such an error term likely would not be i.i.d. and thus would introduce some persistence into our series of the monetary policy shock. In columns 1 and 3 of Table 2.14 in Appendix 2.10.1, we check this issue by regressing the current quarterly shocks against their *past* values, with lags of up to four quarters. We find no evidence of persistence for our constructed series of policy shocks for the euro area and

[8] In particular, monetary policy may respond to housing market conditions. See the discussion in Woodford (2012) on whether central banks should pay attention to the evolution of asset prices and financial stability when making monetary policy decisions, and the empirical evidence in Schularick et al. (2020).

Switzerland, respectively, supporting that our constructed series of policy shocks are true shocks.[9]

The advantage of identifying monetary policy shocks using high-frequency data on market expectations is that one does not need to make further assumptions about policymakers' information set or to impose identifying restrictions, as in the traditional VAR-literature, to disentangle the endogenous and exogenous components of monetary policy. Such assumptions frequently result in shock series for monetary policy shocks that are not easily reconciled with data on financial market expectations (see, for example, the critique by Rudebusch, 1998).

We retrieve market expectations about policy rates by using price data (from *TickDataMarket*) of futures contracts on the policy rate or a close counterpart. The midpoint of the policy rates is the rate on the main refinancing operations of the ECB, which is relevant for Germany and Italy, and the three-month Swiss-Franc Libor rate for Switzerland. Whereas futures are traded for the three-month Swiss-Franc Libor, this is not the case for the rate on the main refinancing operations. Therefore, we use futures on the three-month Euribor. The Euribor is highly correlated with the rate on refinancing operations, as shown in Figure 2.6 in Appendix 2.10.1.[10]

[9] In our analysis, we cumulate shocks for every year. Figure 2.5 in Appendix 2.10.1 shows the correlograms of the series with shocks cumulated over a year. Even for the cumulated series of the shocks, we do not find significant autocorrelations beyond two quarters, which is comforting because the multicollinearity of the lagged shocks in the regressions is not a concern. In columns 2 and 4 of Table 2.14 in Appendix 2.10.1, we check whether *future* cumulated shocks can be predicted by past cumulated shocks. We find that past shocks have no predictive power for future shocks in the euro area. This is also, by and large, the case for the Swiss series, with the exception that past shocks with a lag of three years or more are significant at the 10% level. The sample size is smaller in these regressions because of the longer lags.

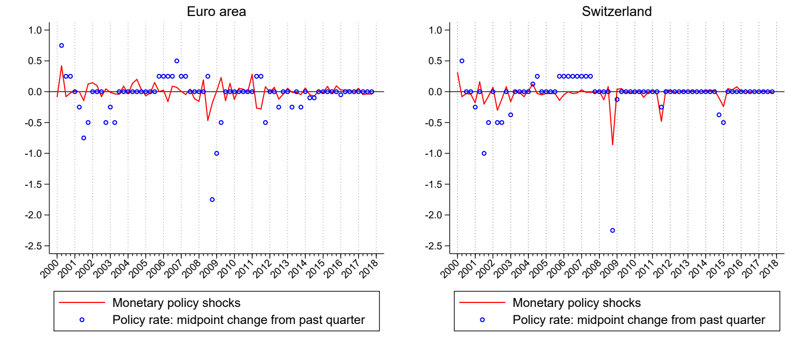
[10] Given that future contracts often mature around the announcement dates, we use futures contracts that deliver a specified rate in the quarter following the monetary policy announcement. These contracts mature after the announcement dates, and we observe the price changes for these contracts around the announcement dates. We do not need to adjust the implied rates of the futures contracts for the number of days until expiry. In Gürkaynak et al. (2005), Nakamura and Steinsson (2018a) or Wong (2021) this is necessary because they use contracts of federal funds futures in the U.S. that have a payout based on the average effective rate in a given month.

We use the futures contracts on the three-month Euribor, not the overnight interest swaps as in Altavilla et al. (2019), as a measure of the interest rate shocks in the euro area because the adjustable-rate mortgages in the euro area use the three-month Euribor as the reference rate. This is analogous to the three-month Swiss-Franc Libor for Switzerland. Using the three-months Euribor also has the advantage that we can use data from 2000 onwards. This would not be possible if we used overnight interest swaps because, as mentioned in Altavilla et al. (2019), the data for the overnight interest swaps are very noisy until 2002.[11]

Figure 2.1 plots our measure of the monetary policy shock constructed from the unexpected futures price changes together with the actual changes in the midpoint policy rate. We cumulate the shocks, which we obtain by computing the rate changes in the narrow time window around each policy announcement, and the corresponding midpoint policy rate changes for all announcements within a quarter. As can be seen in Figure 2.1, changes in the policy rate are partly anticipated. For example, only a small part of the large decrease in the policy rate in 2008 has been unexpected. Instead, on other announcement dates, markets expected a reduction in the policy rate whereas the central bank kept the rate unchanged. This resulted in an unexpected shock reflecting that the policy rate remained higher than expected.

The average of the shocks is approximately zero in the sample period for the ECB and −3 basis points for the SNB. The standard deviation of the shocks is 7 basis

[11] The overnight interest swaps (OIS) use the euro overnight index average (EONIA) as the reference rate. For the same three-month maturity, the monetary policy shocks constructed based on the OIS and the Euribor futures are highly correlated with a correlation coefficient of 0.59 in our sample period. The correlation is not perfect because the series differed in periods of high financial distress, such as the financial crisis and the sovereign debt crisis in the euro area. In these crises, the spread between the EONIA and the Euribor captures the counterparty credit risk, given that lending overnight based on the EONIA is rolled over daily until maturity in the three-month period whereas lending from one counterparty based on the three-month Euribor is not and thus has higher counterparty credit risk. Hence, for these crises episodes, changes in the futures of the three-month Euribor also capture changes in interbank risk premia, which are relevant for mortgage interest rates. Because we want to capture this effect, we use the Euribor futures for our analysis of the transmission of monetary policy shocks to the housing market.



Sources: Short-term rates from ECB (Statistical Data Warehouse, Table *ECB/Eurosystem policy and exchange rates*, Subtable “Official interest rates“) and SNB (Data Portal, Table *Official interest rates*). Futures contracts' prices from *TickDataMarket*. *Notes:* Quarterly data. The series of shocks is constructed using data on futures contracts for the 3-month Swiss-Franc Libor and the Euribor. Both the shocks and the midpoint changes are cumulated quarterly.

Figure 2.1: *Monetary policy shocks and midpoint policy rate changes (%)*

points for the ECB and 10 basis points for the SNB,[12] similar to the 9 basis points reported in Wong (2021) for the Federal Reserve during the 1990 – 2007 period. Given that some shocks in the sample are much larger than others, we check the robustness of our results in Appendix 2.10.2 if we exclude the years 2007 and 2008 and thus the large policy rate shock during the financial crisis. We also check the robustness in Appendix 2.10.2 if we exclude periods with a negative interest rate policy (NIRP), or if we use shocks to long-term yields instead of the policy rate given that long-term yields are positive in the sample period.

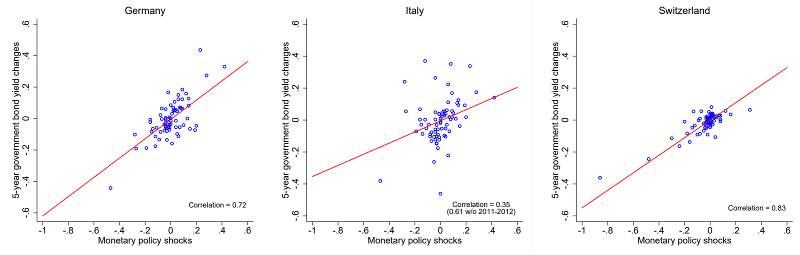
As previously mentioned, we further check the robustness by constructing the shocks using narrower time windows to measure the price changes in the future

[12] The difference in the standard deviation may be related to the different frequency of the regular announcements. The ECB announces rate decisions every six weeks. The SNB announcements have a lower frequency of three months. Tables 2.12 and 2.13 in Appendix 2.10.1 show that the mean and the standard deviation of the shocks increase as we cumulate them within a quarter or year, quantitatively similar to results reported by Wong (2021), Table 1, for the rate shocks of the Federal Reserve in the U.S.

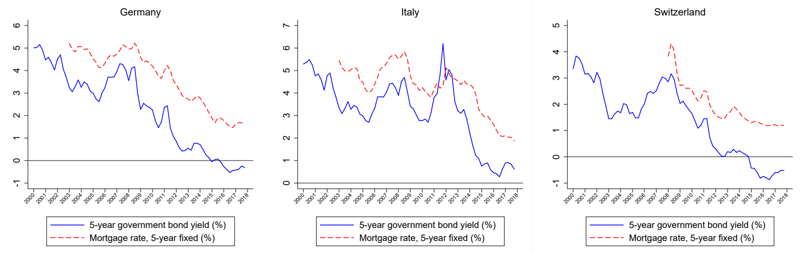
contracts based on data at a minute frequency as provided by *TickDataMarket*. For Switzerland, we consider a very narrow time window of only 30 minutes around the announcement, starting 10 minutes before the announcement. This replicates the identification strategy of Gertler and Karadi (2015) and Nakamura and Steinsson (2018a) for the U.S. Considering such a narrow time window is sensible for Switzerland because press conferences of the SNB after announcements are only held occasionally and, generally, announcements are made available to the public only through the SNB website. Instead, press conferences are common at the ECB. Thus, we also consider a larger time window, which accounts for the fact that monetary policy decisions are communicated slightly differently by the ECB and SNB than the Federal Reserve. As in Corsetti et al. (2021), our measure of shocks is broad, including the various communication channels through which monetary policy announcements affect the economy (Altavilla et al., 2019)

The ECB typically makes an initial policy announcement at 13:45 (CET), in which the policy rate decision is briefly stated. In a subsequent press conference at 14:30 (CET), the decision is explained further. Therefore, we also construct the shocks with a time window from 13:00 to 19:00, as in Corsetti et al. (2021). The SNB announcements are first released on its website, which is directly followed by a press conference only for the quarterly meetings in June and December. The precise time of day of the announcement varies but is known in advance, and the press conference lasts for approximately one hour. The majority of the statements in our sample started between 09:30 and 14:00 (CET).[13] Given the similar structure of the SNB announcements, for the instances in which announcements are followed by a press conference, we also consider a time window of six hours around the announcement time as for the ECB. The results for the responses of housing tenure and rents, using these alternative time windows to measure the monetary policy shocks, are discussed in Sections 2.6 and 2.7 and reported in Appendices 2.10.2 and 2.10.3.

[13] The initial SNB statements started between 08:50 and 17:45 (CET) in our sample. All of the June and December meetings started in the morning. On 06.09.2011, 18.12.2014 and 15.01.2015 extraordinary announcements were followed by a press conference.



(a) Monetary policy shocks and long-term bond yield changes on announcement dates (%)



(b) Long-term bond yields and rates for fixed-rate mortgages (%)

Sources: Rates of five-year fixed-rate mortgages from ECB (Germany *MIR.M.DE.B.A2C.O.R.A.2250.EUR.N*, Italy *MIR.M.IT.B.A2C.O.R.A.2250.EUR.N*) and SNB (*EPB@SNB.zikrepro{M,50}*). Five-year government bond yields from Thomson Reuters (RIC *DEMYT*, *ITMYT* and *CHMYT*, where *MYT* denotes maturity). *Notes:* Panel (a) uses daily changes on announcement dates between 2000Q1 and 2017Q4. Panel (b) displays quarter values for the mortgage rates, and quarterly averaged bond yields.

Figure 2.2: *Monetary policy shocks and long-term interest rates*

2.4 Pass-through to market interest rates

Key for the monetary policy transmission to the housing market is the pass-through of the monetary policy shocks to the mortgage interest rates which affect the user cost of owning a home. The results presented in this section indicate that the shocks indeed have a persistent effect on interest rates and thus pass through to long-term interest rates such as mortgage rates in Germany, Italy and Switzerland. We find that the pass-through to five-year fixed-rate mortgage rates is twice as large in Switzerland than in Germany and Italy.

Our analysis of the pass-through proceeds in the following steps. We first establish that policy rate shocks affect long-term bond yields, on which we have data for the narrow time window around the policy announcement dates. We illustrate the persistence of the pass-through by considering yields with different maturity and we show that the yields of long-term bonds co-move with mortgage interest rates. We then estimate the pass-through of the policy shocks to rates of five-year fixed-rate mortgages, which are available at a monthly frequency. We also show that the policy rate shocks affect the spread between mortgage rates across Italian regions, at a quarterly frequency given the data availability.

2.4.1 Pass-through to long-term yields

Panel (a) of Figure 2.2 shows that our measure of monetary policy shocks for the ECB and SNB, respectively, is highly correlated with changes in the yields of five-year government bonds which are available in the same time window around the announcement dates. Panel (b) of Figure 2.2 illustrates that fixed-rate mortgage rates co-move with long-term bond yields. Fixed-rate mortgage rates are available for part of the sample period and not at the high frequency around the announcement dates.

Table 2.2 provides quantitative evidence on the pass-through of the monetary policy shocks to yields with different maturities. Each number reported in the table corresponds to a coefficient estimate obtained by regressing the interest

Table 2.2: *Persistent effects of monetary policy shocks*

	Euro area				Switzerland
6M Futures' implied rate	0.803***				0.920***
	(0.059)				(0.037)
9M Futures' implied rate	0.853***				0.855***
	(0.058)				(0.047)
12M Futures' implied rate	0.859***				0.786***
	(0.058)				(0.059)
15M Futures' implied rate	0.818***				0.762***
	(0.057)				(0.067)
18M Futures' implied rate	0.779***				0.727***
	(0.057)				(0.076)
21M Futures' implied rate	0.737***				0.709***
	(0.055)				(0.084)
	Germany			Italy	Switzerland
3Y Government bond yield	0.638***			0.528***	0.496***
	(0.062)			(0.087)	(0.057)
4Y Government bond yield	0.609***			0.468***	0.451***
	(0.056)			(0.090)	(0.044)
5Y Government bond yield	0.629***			0.438***	0.412***
	(0.057)			(0.088)	(0.043)
6Y Government bond yield	0.586***			0.406***	0.344***
	(0.055)			(0.074)	(0.051)
	Nominal	*Real*	*Inflation*		
5Y Government bond yield†	0.813***	0.318***	0.495***		
	(0.067)	(0.063)	(0.080)		

Sources: Futures' implied rates from Thomson Reuters (RIC *FEIMYD* and *FESMYD*, where *MYD* denotes month, year and decade). Bond yields from Thomson Reuters (RIC *DEMYT*, *ITMYT* and *CHMYT*, where *MYT* denotes maturity, and ISDN *DE0001030526* for the *Bobl* real bond). *Notes:* Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. †Estimates for the transmission to nominal rates, real rates, and break-even inflation using the 90 monetary policy announcements since the inflation-indexed Bobl bond has been issued in Germany in 2009. Standard errors are in brackets. The table reports the coefficients of separate regressions for each financial instrument against the monetary policy shocks series and a constant for Germany, Italy and Switzerland, respectively. The series are based on daily changes in the rates on the announcement dates during 2000Q1-2017Q4. The number of announcements in the sample period is 87 for Switzerland and 229 for the euro area.

rate of the respective financial instrument on a constant and the monetary policy shock. A coefficient value of 1 corresponds to a full pass-through of the shock (i.e., a shock of 25 basis points translates to a change of 25 basis points in the interest rate of the respective financial instrument).

The estimated regression coefficients reveal that the shocks have persistent effects on interest rates in both countries. At the top of the table, we report the effect on the implied short-term interest rate of future contracts up to 21 months in the future. The effect on these expected short-term rates is easier to interpret than the effect on bonds with longer maturities, reported below in the same table: the effect on the rates of the long-term bonds depends on the average of the effect on short-term rates over the life of the bond and may also be affected by changes in the risk or term premium.[14] The size of the coefficients at short maturities as reported in Table 2.2, and the persistence of the effect of monetary policy shocks on nominal rates, are similar to the estimates for the U.S. reported in Table 1 of Nakamura and Steinsson (2018a). One difference is that the pass-through monotonically decreases for instruments in Switzerland with longer maturity and the pass-through is strongest in the euro area at a maturity of one year. For the U.S., Nakamura and Steinsson (2018a) find that the pass-through is strongest at a maturity of two years.

For Germany, we provide evidence for the effect of monetary policy shocks on real rates for a shorter sample period. Inflation-indexed *Bobl* bonds have been issued only since 2009 and we have a sample of 90 monetary policy announcements

[14] Nakamura and Steinsson (2018a) present evidence that indicates that changes in risk premia are not the main drivers in the transmission of monetary policy shocks, identified by high-frequency variation, on long-term interest rates. The empirical analysis using daily data on yields for Switzerland by Söderlind (2010) suggests that an increase in expected short-term interest rates may confirm the credibility of price stability and thus lead to a *de*crease in long-term rates via a reduced term premium. Without such an effect, the effect of changes in short-term rates on long-term rates would be even larger. For the euro area, changes in risk premia in financial crises and sovereign debt crises explain some of the differences in the pass-through to German compared with Italian government bonds which we observe in Table 2.2. If we exclude the years 2008/9 of the financial crisis and the years 2011/12 of the euro-area debt crisis, then the regression coefficients for Italy are much more similar to the coefficients for Germany, taking the values of 0.635, 0.530, 0.581, and 0.524 for the government bonds with maturities of three, four, five and six years, respectively. Thus, we perform robustness checks in our analysis in which we omit the crises episodes.

since then. We use the available data on five-year nominal and real government bonds because no indexed bonds with shorter maturities are issued. We find that more than one third (39%) of the response of the nominal rate to the monetary policy shock can be attributed to the change in the real rate. The effect on break-even inflation accounts for the remaining response, where break-even inflation is computed as the difference between the nominal and real yields. Compared with the empirical evidence of Nakamura and Steinsson (2018a) for the U.S., we find a stronger positive effect of monetary policy shocks on break-even inflation in Germany. Our results suggest that, on impact in our sample period, markets have revised their inflation expectations upward after an unexpected positive change in the policy rate.

2.4.2 Pass-through to mortgage interest rates

We estimate the pass-through to mortgage interest rates using aggregate data on mortgage rates available at a monthly frequency.[15] We estimate the pass-through to rates of five-year fixed-rate mortgages because this is a representative mortgage type in Germany and Switzerland, and relevant for Italy as well (see Section 2.2). The pass-through for adjustable-rate mortgages is more mechanical because the three-month Euribor and Swiss-Franc Libor are the respective reference rates in these adjustable-rate contracts.

Table 2.3 shows that the pass-through of the policy rate shocks to the rates of five-year fixed-rate mortgages is twice as large in Switzerland than in Germany and Italy. The results in the top part of Table 2.3 imply that an unexpected 25 bp cut in the policy rate reduces the mortgage rate by 22 bp in Switzerland within two months, and only by $10 - 12$ bp in Germany and Italy. Furthermore, the pass-through in Switzerland occurs immediately, i.e., in the same month as the policy rate shock. Most of the pass-through in Germany and Italy occurs a month

[15] The information on mortgage interest rates in the household-level data for Italy is available only at a biannual frequency. The information on mortgage payments, available in the household-level data for Germany and Switzerland, are available at an annual frequency. A disadvantage is that these data contain both quantity and price effects resulting from interest rate changes.

later, following the policy rate shock. Comparing the top and bottom part of the table shows that the pass-through occurs within two months in all three countries. Adding a further lag of the policy rate shock implies only minor changes to the coefficient estimates.

Table 2.15 in Appendix 2.10.1 shows that the pass-through to the mortgage rate increases from 10 bp to 18 bp in Italy, and remains very similar for Germany and Switzerland, if we only consider policy rate shocks that are positively correlated with long-term (government) bond yields. This finding suggests that the pass-through in Italy to mortgage rates was weaker during the euro-area debt crisis, in which the pass-through of policy rate shocks to government bond yields was different because of changes in risk premia. This is illustrated in Figure 2.3 which shows that fixed-rate mortgage rates co-move positively with the rates of long-term government bonds in both financially more and less developed Italian regions, but for the years 2010-2012 of the sovereign debt crisis in the euro area.

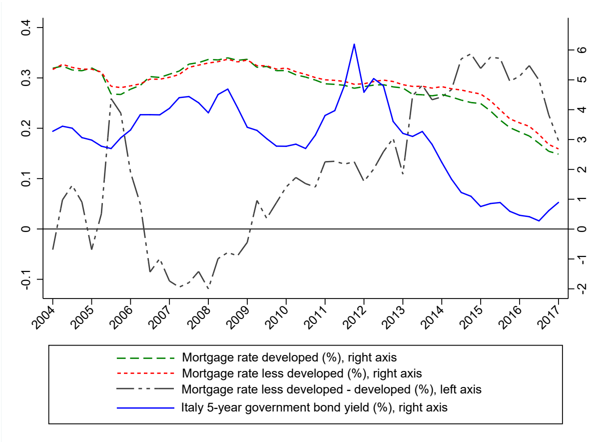
Figure 2.3 further shows that the aggregate mortgage rate in Italy hides sizable regional heterogeneity. Among the three countries considered, these regional differences are specific to Italy. We exploit them for identification in Section 2.8 when we estimate the transmission of policy rate shocks to the housing market across Italian regions. We categorize the Italian regions as financially more or less developed, which is highly correlated with Northern and Southern Italian regions in line with previous research by Guiso et al. (2004) as further documented in Table 2.37 of the data appendix 2.10.5.

Figure 2.3 shows that the spread between the mortgage rate in the less developed and developed regions varies considerably in the sample period, is larger than 30 bp in some sample years, and is negatively correlated with the level of the long-term interest rate. The regression results in Table 2.4 confirm these findings, based on data at a quarterly frequency. The results in column 1 show that the spread of the mortgage rate across Italian regions with different financial development is negatively correlated with changes in long-term bond yields. The results in columns 2 and 3 show that the pass-through of the policy rate shocks to the spread is only economically and statistically significant if we consider policy rate shocks that are positively correlated with long-term yields (column 3). Once we

Table 2.3: *Pass-through of policy rate shocks to five-year fixed-rate mortgage rates*

	Germany	Italy	Switzerland
Monetary policy shocks, sum M(0)	-0.012	-0.155	$0.793^{***\ \dagger}$
Monetary policy shocks, sum M(-1)	0.295^{***}	0.360^{**}	0.014
Monetary policy shocks, sum M(-2)	0.210^{**}	0.210	0.069
Cum. effect (in pp) of 25 bp cut	-0.123^{***}	-0.104	-0.219^{***}
Monetary policy shocks, sum M(0)	-0.021	-0.159	$0.795^{***\ \dagger}$
Monetary policy shocks, sum M(-1)	0.281^{***}	0.386^{***}	0.017
Monetary policy shocks, sum M(-2)	0.231^{**}	0.213	0.067
Monetary policy shocks, sum M(-3)	0.054	0.108	0.049
Cum. effect (in pp) of 25 bp cut	-0.137^{***}	-0.137	-0.232^{***}

Sources: Rates of five-year fixed-rate mortgages from the ECB (Germany *MIR.M.DE.B.A2C.O.R.A.2250.EUR.N*, Italy *MIR.M.IT.B.A2C.O.R.A.2250.EUR.N*) and the SNB (*EPB@SNB.zikrepro{M,50}*). *Notes:* Regression of monthly mortgage-rate changes on policy rate shocks cumulated by month. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The series is based on the monthly changes in the rates available for the 2008M1-2017M12 period in Switzerland and the 2003M1-2017M12 period in the euro area. The cumulative effect over three years of a -25 bp shock is computed by multiplying the sum of the coefficients by -0.25.
† Regular policy announcements at the SNB occur once a quarter. For the months without an announcement the value of the shock is zero.



Sources: Mortgage rates from Banca d'Italia (Statistical Database, Table *Lending rates applied to loans for house purchase (stock) - by initial period of rate fixation, customer region and total credit granted (size classes)*, >= 125,000 euros, over 1 year fixation, Reference *TDB30890*). Five-year government bond yields from Thomson Reuters (RIC *ITMYT*, where *MYT* denotes maturity). *Notes:* The mortgage rate in less developed regions is the average quarterly mortgage rate in Sardinia, Tuscany, Abruzzo and Molise, Basilicata, Sicily, Apulia, Lazio, Campania and Calabria. The mortgage rate in developed regions is the average quarterly mortgage rate in Marche, Liguria, Emilia-Romagna, Veneto, Piedmont, Trentino-Alto Adige, Lombardy, Friuli-Venezia Giulia and Umbria.

Figure 2.3: *Long-term interest rates and regional mortgage rates in Italy*

Table 2.4: *Effect of bond yield changes and monetary policy shocks on mortgage-rate spread across Italian regions*

	Mortgage rate less developed - developed		
Q-o-q 5y Italian bond yield change, sum Q(-1;-4)	-0.051***		
Q-o-q 5y Italian bond yield change, sum Q(-5;-8)	-0.050**		
Q-o-q 5y Italian bond yield change, sum Q(-9;-12)	-0.024		
Monetary policy shock, sum Q(-1;-4)		-0.007	
Monetary policy shock, sum Q(-5;-8)		0.017	
Monetary policy shock, sum Q(-9;-12)		-0.044	
Effective monetary policy shock, sum Q(-1;-4)			-0.186**
Effective monetary policy shock, sum Q(-5;-8)			-0.152*
Effective monetary policy shock, sum Q(-9;-12)			-0.171**
Year dummies	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes
Observations	53	53	53
Adjusted R^2	0.85	0.82	0.84
Cum. effect (in pp) of 25 bp cut	0.03**	0.01	0.13**

Sources: Regional mortgage rates from Banca d'Italia (Statistical Database, Table *Lending rates applied to loans for house purchase (stock) - by initial period of rate fixation, customer region and total credit granted (size classes)*, >= 125,000 euros, over 1 year fixation, Reference *TDB30890*). Bond yields from Thomson Reuters (RIC *ITMYT*, where *MYT* denotes maturity). *Notes:* Regression of difference between average mortgage rate difference between less developed and developed Italian regions against bond yield changes and monetary policy shocks for 53 observations between 2004Q1 and 2017Q1. The regression using effective monetary policy shocks only uses those monetary policy shocks that have the same sign as the Italian five year government bond yield change on announcement dates. The mortgage rate in less developed regions is the average quarterly mortgage rate in Sardinia, Tuscany, Abruzzo and Molise, Basilicata, Sicily, Apulia, Lazio, Campania and Calabria. The mortgage rate in developed regions is the average quarterly mortgage rate in Marche, Liguria, Emilia-Romagna, Veneto, Piedmont, Trentino-Alto Adige, Lombardy, Friuli-Venezia Giulia and Umbria. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

implicitly exclude the euro-area debt crisis episode, in which the policy rate shocks have been less effective in passing through to long-term yields, an unexpected 25 bp cut in the policy rate increases the spread by decreasing the mortgage rate by 13 bp more in financially developed Italian regions.

2.5 Household data on housing markets

We use household-level data to analyze the transmission of monetary policy to the housing market. Given that we have shown in the previous section that policy rate shocks pass through to mortgage interest rates and thus affect the user costs of households, the household-level data allow us to investigate in detail the gross flows across housing tenure states, the pass-through to rents and house prices, and the heterogeneity of this pass-through across households with different ages, incomes, and net worth. Because we have information on house prices only from the Italian and German household-level data, we provide evidence for Switzerland on the pass-through to the price-rent ratio based on aggregate data.[16]

We use microdata from the German Socioeconomic Panel (SOEP), the Italian Survey of Household Income and Wealth (SHIW), and the Swiss Household Panel (SHP). For Switzerland we complement the panel data with repeated cross-sectional data on rents from the Swiss household budget survey (HABE). For Germany and Italy, information on rents is available in the SOEP and SHIW. Further recent descriptions of the data are provided by Goebel et al. (2019) for the SOEP, the Bank of Italy for the SHIW,[17] Voorpostel et al. (2017) for the SHP and BFS (2013) for the HABE.

Because households in the annual surveys for Germany and Switzerland are interviewed across all quarters and the sample size is sufficiently large, we can use variation across quarters during the period $2000Q1 - 2016Q4$. Because of the lagged independent variables in the estimations, the sample for the estimation

[16] For Germany we approximate the house value using information on mortgage payments, as explained in data appendix 2.10.5.

[17] See https://www.bancaditalia.it/statistiche/tematiche/indagini-famiglie-imprese/bilanci-famiglie/index.html .

starts in 2003$Q1$ for both countries. The sample size is $138,682$ for Germany, and $45,816$ and $22,918$, respectively, for the samples obtained from the SHP and HABE datasets in Switzerland. The unit of observation is a household interviewed in a quarter of a given year. For Italy the sample size is $27,896$ and the biannual survey frequency requires that we exploit variation across years during the same sample period. The Italian data have the advantage that we can exploit in our analysis the regional heterogeneity in the pass-through to mortgage rates and the housing market. We thus obtain further insights by using these regional differences to identify the transmission of monetary policy to the housing market. Before we move on to the analysis of the transmission, we provide descriptive evidence on some key characteristics of the sample that we use for our analysis. We refer to the data appendix 2.10.5 for further details on the data sets and the sample construction.

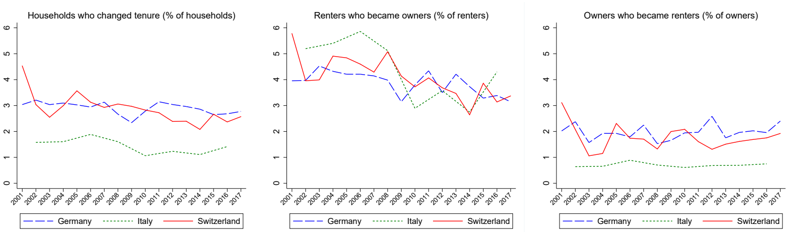
Table 2.5: *Homeownership and mortgage debt by age group*

Ownership rate (%)	**Germany**	**Italy**	**Switzerland**
Aged 25-44	36.8	62.0	38.5
Aged 45-64	58.2	79.6	61.8
Aged 65-84	60.0	84.6	58.7
Incidence of mortgagors (as % owners)	**Germany**	**Italy**	**Switzerland**
Aged 25-44	78.5	36.5	81.9
Aged 45-64	53.9	20.1	80.9
Aged 65-84	16.4	4.9	67.4

Sources: SOEP (Germany), SHIW (Italy), SHP (Switzerland). *Notes:* Given the data availability, the incidence of mortgagors covers the 2002-2016 period for Germany, 2010-2016 for Italy, and 2014-2016 for Switzerland. See Appendix 2.10.5 for further details on the construction of the variables and the sample.

Table 2.5 displays in the top panel the familiar age profile of homeownership (of the main residence) in Germany, Italy and Switzerland. As mentioned in Section 2.2, the homeownership rates in Germany and Switzerland are lower than in Italy.[18] Table 2.5 shows that this is true at all ages and that the ownership rate increases in all countries until retirement. In Switzerland, the ownership rate falls slightly for retired households which relates to the stronger response of the flow from owning to renting to policy rate shocks that we report for Swiss households in the next Section 2.6.

The bottom panel of Table 2.5 shows that the incidence of mortgage debt is lower in Italy than in Germany and Switzerland at all ages.[19] During retirement the incidence of mortgage debt is much larger in Switzerland than in Germany and Italy where most households amortize their mortgage until retirement and then own their home outright. This different amortization behavior in Switzerland is related to different tax incentives for amortization as further analyzed in Koeniger et al. (2021b).



Sources: SOEP (Germany), SHIW (Italy), SHP (Switzerland). *Notes:* Annual average flows. For Italy, biannual flows are annualized. Appendix 2.10.6 contains detailed information about how the flows are annualized.

Figure 2.4: *Housing tenure flows over time*

[18]The averages of the owner occupation rate reported in Table 2.1 do not match exactly the averages across age groups based on the household-level data reported in Table 2.5 because they are based on a different data source and period.

[19]This pattern is robust if we restrict the sample to new owners, i.e., renters who became owners between the last and the current survey wave. The sizes of the subsamples are much smaller then, between 27 and 128 for the age groups shown in Table 2.5.

The age profiles of the homeownership rates in the top panel of Table 2.5 suggest that households change housing tenure status. Figure 2.4 provides explicit information on the transition rates. The left plot displays the percentage of households that have changed housing tenure between the current and previous survey wave. Figure 2.4 also provides information on the separate flows, from renting to owning and vice versa. The middle plot shows the renters who have become owners (as a percentage of the sample of renters), and the right plot shows the owners who have become renters (as a percentage of owners).

The plots in Figure 2.4 show that twice as many households change housing tenure per year in Germany and Switzerland than in Italy. On average around 4% of renters per year become homeowners in all three considered countries.[20] The percentage of owners that become renters per year is lower on average, about 2% in Germany and Switzerland. In Italy, homeownership seems more like an absorbing state, given that less than 1% of owners become renters. We exploit the variation in the flows between different types of housing tenure, across quarters and years, to identify the effect of the monetary policy shocks on changes in housing tenure.

Table 2.6 provides summary statistics for the different housing tenure groups in Germany, Italy and Switzerland. As noted by Andrews and Sánchez (2011a,1), the marginal home buyers and sellers in Germany, Italy and Switzerland may be different because of differences in tax incentives and regulation associated with differences in house prices (see also the references therein). To shed light on the characteristics of the households that change housing tenure status, we distinguish renters that have remained renters (since the last survey) from renters that have become homeowners, and we distinguish homeowners that have remained owners from those that have become renters. Table 2.6 shows that, as one would expect, renters that have become homeowners tend to be younger than those who have remained renters. They have higher incomes, are more likely to work, and have higher net worth (in Germany and Italy, for which data on net worth are

[20] The transition rates are annualized for Italy given the biannual frequency of the survey, as explained further in Appendix 2.10.6. When comparing the transition rates from rental to owning across countries, one has to consider that fewer households rent in Italy than in Germany and Switzerland. The transition rates in the considered countries are approximately half of those in the U.S. reported by Ma and Zubairy (2021), Figure 4, once the rates they report are annualized to make them comparable.

Table 2.6: *Summary statistics for housing-tenure groups in Germany, Italy and Switzerland*

	Germany			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	64,782	2,649	69,887	1,364
Age (household head)	49.2	46.2	55.5	55.5
Household size (persons)	2.34	2.8	2.7	2.4
In a couple (%)	57.0	78.1	79.8	60.5
Married (%)	44.6	62.5	75.0	51.0
Working (%)	64.6	74.4	63.2	57.8
Gender (% male)	50.41	54.7	65.6	51.8
Domestic citizenship (%)	90.6	92.8	96.5	95.2
Gross household income (2010 EUR, annual)	26,969	46,447	45,134	27,714
Net worth (2010 EUR)	15,974	69,201	120,000	49,788

	Italy			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	5,203	474	21,905	314
Age (household head)	55.8	54.2	60.3	56.7
Household size (persons)	2.5	2.5	2.6	2.0
In a couple (%)	57.4	58.4	70.9	39.2
Married (%)	56.5	56.5	70.3	37.9
Working (%)	45.1	52.3	39.2	48.1
Gender (% male)	52.6	57.8	60.8	51.9
Birth region domestic (%)	87.3	94.3	98.1	93.9
Net household income (2010 EUR, annual)	16,935	25,903	32,151	16,335
Net worth (2010 EUR)	4,660	157,789	230,454	5,516

	Switzerland			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	20,219	820	24,369	408
Age (household head)	50.6	45.2	55.9	55.2
Household size (persons)	2.2	2.8	2.8	2.1
In a couple (%)	53.5	78.0	79.9	52.7
Married (%)	41.8	61.5	75.4	43.4
Working (%)	68.9	80.1	65.9	62.7
Gender (% male)	36.8	37.3	39.7	38.2
Domestic citizenship (%)	88.1	90.0	93.7	95.6
Gross household income (2010 CHF, annual)	93,500	127,259	121,704	97,451

Sources: SOEP (Germany), SHIW (Italy), SHP (Switzerland). *Notes:* Averages for households interviewed between 2002 and 2016. Medians for income and net worth. Changes in tenure refer to changes since the last survey. In 2007Q4, a euro was worth 1.45 US-$ and a Swiss Franc was worth 0.87 US-$. Real incomes and net worth are deflated by the national CPI. The datasets do not contain net worth information for Switzerland, and net instead of gross income for Italy. See Appendix 2.10.5 for further details on the construction of the variables and the sample.

available). In Germany and Switzerland, the size of households that have become homeowners is larger and these households are more likely to be composed of married individuals or those living as a couple. The transition from homeownership to rental occurs at later ages, on average previous to retirement. Table 2.6 shows that owners that become renters have relatively less income and lower net worth (in Germany and Italy, for which data on net worth are available). They are less likely to be married or to live as a couple, implying smaller household sizes.

Across countries, renters that become owners are older in Italy than in Germany and Switzerland which may be related to household formation in Italy occuring later in the life cycle. Moreover, the differences in net-worth positions associated with changes of housing tenure are larger in Italy than in Germany. To understand this further, we inspect the net-worth position of households in Italy in the survey wave previous to the change of their housing tenure. We find that the median net worth of renters who have become owners is $7,505$ euro, which is only somewhat larger before the transition than the net worth of households that remained renters. Moreover, the median net worth of $160,539$ euro held by owners, before they become renters in the subsequent survey wave, has the same order of magnitude as the net worth of households that have remained owners. The large amount of additional wealth that renters report when they become owners, and the much smaller amount of wealth which owners report after they become renters, suggest that transfers across households, possibly across generations, are associated with housing tenure transitions in Italy. This evidence is in line with the much lower incidence of mortgages in Italy that we have reported in Table 2.5. Beyond these differences, the characteristics of the respective subpopulations appear quite similar across the three considered countries. Table 2.38 in Appendix 2.10.5 shows that this also applies to Italian regions with different financial development.

The patterns in the characteristics of the marginal populations that change housing tenure status suggest that the pass-through of policy shocks to housing tenure transitions may be heterogeneous, for example across groups with different ages, incomes, or net worth. In our analysis, we thus allow for a heterogeneous pass-through in some specifications.

2.6 The response of housing tenure

In this section we estimate the effect of monetary policy shocks on housing tenure in Germany, Italy and Switzerland. Because the shocks may induce home purchases or sales, we estimate the effect on both the transition from being a renter to becoming a homeowner and vice versa. Homeownership refers to owner occupation of the primary residence in the data sets and does not include ownership of second homes.

We find that a monetary policy shock triggers adjustments in the housing market: some renters become homeowners and, simultaneously, some homeowners become renters. The net effect on owner occupation is positive for an accommodative shock, suggesting that the positive demand effect resulting from such a shock does not only imply higher house prices. We now present our findings in further detail.

We exploit variation at a quarterly frequency for Germany and Switzerland because we have information on the interview date of households. For Italy we use the annual variation in the shocks and biannual transitions given the lower survey frequency. We discuss the resulting differences in the subsequent regression specifications. The reported cumulated effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions, as explained further in Appendix 2.10.6.

Given that households in the German and Swiss panel data are interviewed at an annual frequency, we pool all of the observations on renters to estimate the probability of becoming a homeowner in each quarter and year, and we pool all of the observations on homeowners to estimate the probability of becoming a renter. Households who change housing tenure more than once are captured at each change. Age controls in the regression account for differences in the transition probabilities across age groups.

We use the panel dimension of the surveys to construct a dummy variable for changes in housing tenure during the last year. For household i from region r interviewed in quarter q and year t we define

$$\text{Change}_{irqt} = \begin{cases} 1 & \text{if the housing tenure changed,} \\ 0 & \text{otherwise.} \end{cases}$$

We estimate a linear probability model and provide robustness results for non-linear probit and logit specifications in Appendix 2.10.2. The regression specification is

$$\text{Change}_{irqt} = \alpha + \beta' \mathbf{z}_{qt} + \gamma' \mathbf{x}_{irqt} + D_r + D_q + D_t + \varepsilon_{irqt},$$

where Change_{irqt} is the binary variable previously described, and the vector $\mathbf{z}_{qt}$ denotes the monetary policy shocks in the last three years, cumulated over quarters separately for each of the years.[21] The vector $\mathbf{x}_{irqt}$ contains a set of control variables, which vary at the household level.[22] In all of the regression specifications we control for common effects by quarter D_q and year D_t, and thus control for common trends and seasonal effects. In some specifications we also control for common effects by region D_r, or we allow for heterogeneous effects across population groups with different ages, incomes, and wealth. The identification of these effects exploits the cross-sectional variation in the household data.

[21] For Italy the regression specification modifies to

$$\text{Change}_{irt} = \alpha + \beta' \mathbf{z}_t + \gamma' \mathbf{x}_{irt} + \delta t + D_r + \varepsilon_{irt},$$

given that the survey frequency requires that we cumulate shocks $\mathbf{z}_t$ by year. Thus, the coefficients β are identified by annual variation where we control for a common linear time trend. In the specifications, in which we identify the effect of policy rate shocks by exploiting regional differences, we add interactions of the shocks $\mathbf{z}_t$ with a dummy for financially developed regions and control for aggregate time effects more flexibly by adding time dummies. The coefficients of interest are then identified by the region-year variation.

[22] We do not use aggregate variables as controls because doing so would contaminate our regression specification. For example, unemployment and real GDP growth affect monetary policy decisions and, simultaneously, are influenced by them so that these variables are endogenous. If our constructed monetary policy shocks are exogenous and thus are true surprises, which we have attempted to achieve with our construction of the series, omitted variables are uncorrelated with these shocks and do not bias the coefficient estimates.

The estimation of our specification is straightforward given that the monetary policy shocks have been constructed to be exogenous. The year-quarter variation identifies the effect of the monetary policy shocks in our regressions for Germany and Switzerland. In the specifications for Italy, identification is based on the annual deviations from the trend, and on the region-year variation for the specifications presented in Section 2.8. To preserve degrees of freedom, we estimate a parsimonious specification. We cumulate shocks per year and allow for lagged effects of shocks up to three years. In Appendix 2.10.2 we show that including additional lags of the shocks amplifies the benchmark results for the transitions from renting to owning that we subsequently present, at the cost of less degrees of freedom, such that the main specification provides conservative estimates.[23]

Table 2.7 summarizes the results for the effect of monetary policy shocks on housing tenure in Germany, Italy and Switzerland. In the benchmark specification reported in Table 2.7, the different data frequencies across countries, as previously explained, imply that we control for year and quarter dummies for Germany and Switzerland and a linear time trend for Italy. We cluster standard errors by quarter of interview for Germany and Switzerland and by year for Italy because the monetary policy shocks do not vary at the household level. In Appendix 2.10.2 we show that our results are robust if we add additional controls for household characteristics on which we presented descriptive evidence in Table 2.6. Thus, the observable heterogeneity in the sample composition in each of the considered countries does not explain the different transmission of monetary policy to the housing market across countries which we find.

Table 2.7 shows that the monetary policy shocks affect housing tenure choices significantly in all countries.[24] The quantitative patterns are quite different though.

[23] For Italy we can include less additional lags because we have less degrees of freedom. Given that the additional lags reduce the length of the sample period that we can use for our estimation, we display estimates in Table 2.22 in Appendix 2.10.2 for the benchmark specification on the smaller sample for ease of comparison, together with the estimates for the specifications with the additional lags of the shocks.

[24] The low adjusted R^2 in Table 2.7 illustrates that much of the variation at the household level remains unexplained. This is not surprising because the only variable, that enters the regressions and varies at the household level, is age. Because our goal is to estimate the causal effect of policy rate shocks and not to predict the transitions, we have refrained from adding more variables in our benchmark specification that may improve the

Table 2.7: *Effect of monetary policy shocks on housing tenure transitions*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.019	-0.024**	-0.032***	0.002	-0.003*	-0.017***
Monetary policy shock, sum Q(-5,-8)	-0.030*	0.065**	-0.017***	0.001	0.008	-0.004
Monetary policy shock, sum Q(-9,-12)	-0.025*	0.069***	-0.004	0.002	0.007**	0.003
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	67,431	5,677	21,039	71,251	22,219	24,777
Adjusted R^2	0.00	0.01	0.01	0.00	0.00	0.01
Cum. annualized effect (in pp) of 25 bp cut	1.82*	-1.37***	1.32***	-0.13	-0.15*	0.46***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions.

The immediate effect on housing tenure transitions within a year of the shocks is strongest in Switzerland for the transitions from rental to owning and vice versa. This is in line with our finding in subsection 2.4.2 that the pass-through of policy rate shocks to mortgage rates is strongest in Switzerland. The implied change of the user cost seems particularly relevant for younger households for their transition from rental to owning as subsequently discussed. The transitions from owning to renting seem driven more by the effect of the policy rate shock on price-rent ratios, given that this transition is more relevant for older households who are relatively less leveraged. As is is shown in the next Section 2.7, rents in Switzerland respond immediately and most strongly to the policy rate shocks, possibly because of the indexation of rents to mortgage rates discussed in Section 2.2.

The coefficients of the shocks with further lags in Table 2.7 reveal that the policy rate shocks affect the timing of housing tenure transitions in Italy but the effects are not persistent as in Switzerland and Germany. This is illustrated by the opposite sign of the coefficients of the shocks at short and longer lags in Italy compared with the same sign of these coefficients in Germany and Switzerland. In Germany, the policy rate shocks only affect the transition from owning to renting, and more so at longer lags. As we document in Section 2.7, this pattern is similar for the effect of the interest rate shocks on rents and house prices which is stronger at longer lags in Germany than in Italy and Switzerland.

In the bottom row of Table 2.7, we report the total effect of an unexpected 25 bp interest rate cut.[25] We find that the transition from renting to owning increases by $1-2$ pp in Germany and Switzerland. In Italy, the initial increase is more than offset over time, implying a decrease in the transition by 1.4 pp. The negative total effect in Italy becomes smaller (in absolute terms) and is insignificant if we use shocks to long-term yields on announcements dates instead of policy rate shocks, as shown in Table 2.19 in Appendix 2.10.2. The pass-through of policy rate shocks to rates of long-term bonds and mortgages has been less effective in

predictive power at the cost of introducing endogeneity issues. As previously discussed, omitted variable bias is not a concern given the exogeneity of the shocks.

[25] Note that we use a change by 25 bp for the quantitative illustration. We do this because the response to shocks of this size is usually reported. The typical unexpected shock to the policy rate is much smaller in our sample period, as shown in Figure 2.1 in Section 2.3.

Italy during the sovereign debt crisis in our sample period, as discussed in Section 2.4.2.

Thus, the robust finding for Italy is that the transmission of interest rate changes to housing tenure transitions is weaker than in Germany and Switzerland, a result associated with the strong pass-through to house prices in the quarters after an interest rate cut, as shown in Table 2.10 in Section 2.7. Moreover, the descriptive evidence in Table 2.6 in Section 2.5 suggests that other determinants, such as (intergenerational) transfers across households, are associated with housing tenure transitions in Italy, which is in line with the much smaller incidence of mortgages. Thus, the smaller pass-through of monetary policy shocks to housing tenure transitions in Italy may not be surprising after all. Interestingly, the *relative* effect of the monetary policy shocks on housing tenure transitions across Italian regions with different financial development is more similar to the aggregate effects on housing tenure transitions estimated for Germany and Switzerland, as discussed further in Section 2.8.

The bottom row of Table 2.7 further shows that an unexpected reduction in the interest rate increases the transition from owning to renting by 0.5 pp in Switzerland. For Germany and Italy, we find no strong effect on the transition from owning to renting. It decreases slightly and not significantly for Germany. For Italy, the decrease by 0.15 pp is only significant at the 10%-level.

When interpreting the size of the effects, it is important to recall that typical policy rate shocks are much smaller than 25 bp. As mentioned in Section 2.3, the standard deviation of the monetary policy shocks is 7 bp for the ECB and 10 bp for the SNB. Thus, the effects on the transitions reported in Table 2.7 must be scaled down by approximately one third if we consider a policy rate shock of one standard deviation. The scaled effects remain quantitatively relevant given that around 4 percent of renters become homeowners per year and $1-2$ percent of owners become renters, as we have shown in Figure 2.4 in Section 2.5.

Table 2.8 provides evidence on the heterogeneity of the transmission across groups with different ages, incomes and net worth. Information on the latter is only available in the German and Italian data. We choose to report the heterogeneity

Table 2.8: *Cumulative effect of 25 bp cut on housing tenure transitions for groups with different ages, income and net worth*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Whole sample	1.82*	-1.37***	1.32***	-0.13	-0.15*	0.46***
By age group						
Age 25-44	2.94**	-1.59	1.14**	-0.17	-1.15***	-1.23*
Age 45-64	1.06	-0.77**	1.69***	-0.26	-0.06	1.41***
Age 65-84	-1.94	-1.92**	0.95***	-0.19	0.03	-0.26
By income, working age group (25-64),						
Working age group (25-64)	2.35**	-1.11***	1.28***	-0.16	-0.29**	0.75***
Incomes < median	0.52	-1.34***	0.58***	1.47*	-0.32*	-0.10
Incomes > median	3.77**	-0.72	1.82***	-0.82**	-0.20**	1.07***
Incomes < median (within age group):						
Age 25-44	0.25	-1.78*	0.16	6.38***	-2.15**	-4.14*
Age 45-64	0.31	-1.03*	0.89***	-0.78	0.11	1.77***
Age 65-84	-1.19	-1.37	-0.91**	-0.64	0.06	2.35***
Incomes > median (within age group):						
Age 25-44	4.69**	-0.94	1.88**	-2.48**	-0.48*	-0.04
Age 45-64	1.93	0.24	1.59**	-0.01	-0.12***	1.41***
Age 65-84	-3.10	-2.55	4.35***	0.27	0.06	-2.02***
By net worth, working age group (25-64),						
Working age group (25-64)	2.35**	-1.11***	-	-0.16	-0.29**	-
Net worth < median	1.32	-0.21	-	-0.44	-1.07***	-
Net worth > median	2.59**	-10.82***	-	-0.04	0.02	-
Net worth < median (within age group):						
Age 25-44	2.92**	-0.79***	-	-0.50	-7.64***	-
Age 45-64	0.31	0.17	-	-0.28	-0.44	-
Age 65-84	-2.44	-0.34	-	-1.15	0.07	-
Net worth > median (within age group):						
Age 25-44	2.70*	-7.49	-	0.13	-0.23**	-
Age 45-64	1.87	-14.40***	-	-0.24	0.11**	-
Age 65-84	-1.24	-4.39	-	0.02	0.05**	-

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The reported cumulative annualized effect is in pp. The estimates for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions. Subgroups consist of at least 105 observations. The typical subgroup has more than 1,000 observations.

for age, income and net worth because the descriptive evidence presented in Table 2.6, Section 2.5, suggested that the subpopulations that change housing tenure status differ in these dimensions from the rest of the population. Furthermore, age, income and net worth are state variables in typical structural models of household financial behavior so that they are of particular interest. Table 2.8 displays the cumulated effects of an unexpected 25 bp cut by subgroup. The effect reported in each cell of the table is obtained by estimating the benchmark regression specification for each subgroup. The results for the entire sample are repeated in the first row for ease of comparison.

Table 2.8 confirms the suggestive descriptive evidence in Table 2.6. The results in columns 1 to 3 show that the policy rate shocks in Germany and Switzerland trigger more transitions from rental to owning for relatively younger households, for households with income higher than the median, and for households with a higher net worth (for Germany). The patterns for Italian households are less clear cut which is related to the finding that the response of the transition to homeownership has a more complicated timing and is different to Germany and Switzerland, as shown in Table 2.7.

Regarding the transition from owning to renting, columns 4 to 6 in Table 2.8 show that the sizable increase after a policy rate cut in Switzerland is caused by households with a head between ages 45 and 64 before retirement, particularly if they have a lower than median income. For Italy, a policy rate cut reduces the transition from owning to renting for young households with lower income and net worth. Thus, the different response of the transition from owning to renting in Switzerland and Italy seems to be related to the higher homeownership rates of young households in Italy relative to Switzerland.

Given that some research has emphasized the importance of cultural factors for financial decisions of households, we exploit the diversity of languages within Switzerland to assess whether the patterns found across countries are also present within Swiss cantons across language groups. We assign the membership in a language group according to what the household head considers as the first language.[26] The reference language is Swiss German, and the dummies *German*, *French* and *Italian* refer to households in Switzerland with a German, French and Italian mother tongue, respectively. We add household controls to the regression because the descriptive statistics displayed in Tables 2.35 and 2.36 in Appendix 2.10.5 show that language groups differ in observables such as age and income which may affect the response to monetary-policy shocks. We add canton dummies as further controls to capture differences across these cantons, for example in terms of regulations, and we also add canton-year dummies to capture changes in these regulations over time.

Table 2.9 shows that our benchmark results are robust to adding these additional controls, and that housing tenure transitions respond similarly to monetary policy shocks across language groups within Swiss cantons. For ease of comparison, columns 1 and 3 of Table 2.9 display the estimates for the effect of the monetary policy shocks on housing tenure transitions if we just add the household controls to the benchmark specification. In columns 2 and 4 of Table 2.9 we add interactions of the monetary policy shocks with dummies for the respective language group, and control for canton and canton-year fixed effects. The joint F-test for the significance of the language-shock interactions has a p-value higher than 18% for the regression on the transitions from renting to owning in column 2. For the regression on the transitions from owning to renting reported in column 4, households with French and Italian mother tongue do not respond differently to households with a Swiss-German mother tongue. The F-test for each group has a p-value higher than 42%. The only heterogeneity in the housing tenure responses across language groups which we detect is that households with a German mother tongue are more likely to transit from owning to renting after a interest rate cut than households with a Swiss-German mother tongue. The p-value of the F-test is 1% when we test the joint significance of the coefficients for the German-language interactions with the monetary policy shocks.

[26] The first language is reported for 19,474 out of 45,816 households. To ensure a sufficient sample size across quarters in the considered period, we assign the language according to the location of the residence for the remaining households. In Appendix 2.10.5, Tables 2.35 and 2.36 show that the subsample of households who report a first language has similar observable characteristics across language groups as does the sample that we use.

Table 2.9: *Effect of monetary policy shocks on housing tenure, across language groups in Switzerland*

	Renter to owner		Owner to renter	
	(1)	(2)	(3)	(4)
Monetary policy shock, sum Q(-1,-4)	-0.029***	-0.036***	-0.018***	-0.020***
Monetary policy shock, sum Q(-5,-8)	-0.013***	-0.007	-0.008***	-0.008*
Monetary policy shock, sum Q(-9,-12)	-0.002	0.005	0.000	0.007
Monetary policy shock, sum Q(-1,-4) × German		-0.023		-0.004
Monetary policy shock, sum Q(-5,-8) × German		0.035		-0.048**
Monetary policy shock, sum Q(-9,-12) × German		0.028		-0.049
Monetary policy shock, sum Q(-1,-4) × Italian		-0.022		-0.015
Monetary policy shock, sum Q(-5,-8) × Italian		-0.035		0.002
Monetary policy shock, sum Q(-9,-12) × Italian		-0.050		-0.003
Monetary policy shock, sum Q(-1,-4) × French		0.024		0.013
Monetary policy shock, sum Q(-5,-8) × French		-0.019		0.003
Monetary policy shock, sum Q(-9,-12) × French		-0.030		-0.022
Household controls†	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Canton dummies	No	Yes	No	Yes
Canton-year dummies	No	Yes	No	Yes
Observations	21,039	21,039	24,777	24,777
Adjusted R^2	0.02	0.04	0.02	0.02
Cum. effect (in pp) of 25 bp cut	1.10***	0.95**	0.66***	0.53**

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. †: Household controls also use information on the household's reference person and include: age, age squared, household size, civil status, partnership, working status, gender, nationality, real household income. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Language interactions use the first language of the household's head; the reference language is Swiss-German. Standard errors are clustered by quarter of the interview, because the monetary policy shock does not vary at the household level. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

Overall, we find little evidence for different responses across language groups within Switzerland. This finding suggests that cultural differences may not be of first-order importance for understanding the cross-country differences in the pass-through of monetary policy shocks to housing tenure transitions, which we reported in Table 2.7. Instead, the differences in housing markets across these countries discussed in Section 2.2, and the different pass-through of the monetary policy shocks to the mortgage interest rates discussed in Section 2.4.2 seem to play an important role.

2.6.1 Robustness

Appendix 2.10.2 contains all of the robustness checks for the main regression specification reported in Table 2.7. We discuss the results to which we have not yet referred. In Appendix 2.10.2, we provide robustness checks for alternative time windows around the policy announcements. Specifically, we construct policy rate shocks using six-hour time windows as in Corsetti et al. (2021) for constructing policy rate shocks, and 30-minute time windows as in Nakamura and Steinsson (2018a). Table 2.16 in Appendix 2.10.2 shows that the correlation between these shocks series and our benchmark series is high except for the series based on 30-minute time windows in the euro area. The communication of monetary policy by the ECB requires longer time windows than 30 minutes to capture all of the new information released. Thus, we report robustness checks for the euro-area countries Germany and Italy using the series for monetary policy shocks based on a six-hour time window, as in Corsetti et al. (2021). For Switzerland, we report the robustness results for both the shock series based on the six-hour and the 30-minute time window.

Tables 2.17 and 2.18 in Appendix 2.10.2 show that the estimated effects on housing tenure transitions are robust to using the alternative time windows. The coefficient estimates tend to become larger if we use shocks based on shorter time windows. This result should be expected given the timing of the announcements and the subsequent press conferences which we have described in Section 2.3. The shocks are smaller if measured over shorter time windows because the full effect of the announcement takes longer to pass through to the futures market, particularly so for the very short 30-minute time window. We view the benchmark time window of one day, between the end of the announcement day and the day before, as a reasonable compromise between capturing the full effect of the monetary policy announcement on the futures prices and avoiding that other changes in that time window confound the results.

Table 2.19 in Appendix 2.10.2 shows that the responses of housing tenure transitions from renting to owning are very similar in Germany and Switzerland if we use unexpected changes in the five-year government bond yields on the announcement days. For Italy, the responses become less different to Germany and Switzerland compared with our benchmark results. The remaining difference in the responses of housing tenure transitions between Italy and the other two countries in Table 2.19 show that not only the transmission from policy rates to long-term yields, documented in Section 2.4.2, but also the transmission from long-term yields to housing tenure transitions is different in Italy relative to Germany and Switzerland.

Table 2.20 in Appendix 2.10.2 and Table 2.21 in Appendix 2.10.2, show that our results are robust if we exclude the years 2007/2008 of the financial crisis from our sample or the quarters with a negative interest rate policy (NIRP). The latter results suggest that the effect of monetary policy shocks on housing tenure transitions does not differ substantially in environments with low interest rates. To put this finding into perspective, note that recent research by Berger et al. (2021) and Eichenbaum et al. (2018) shows that the transmission of monetary policy depends on the interest rate level in the U.S. as a result of refinancing and prepayment of fixed-rate mortgages. This channel seems less important for the countries considered in our analysis, in which refinancing and prepayment of mortgages are much more costly. Moreover, the refinancing decision directly affects indebted homeowners and their expenditures but is less relevant for the decision of renters to become homeowners.

Table 2.25 in Appendix 2.10.2 provides evidence that the effects of monetary policy shocks depend on the sign of the shocks. In Germany, unexpected rate cuts have a stronger effect on the transition from owning to renting but a more moderate effect on the transition from renting to owning. For Switzerland the effect of the unexpected rate cuts has a stronger effect on the transition from renting to owning instead. For Italy, unexpected rate increases have stronger effects on the housing tenure transitions. Evidence that is not reported for brevity shows that these asymmetries are associated with asymmetries in the transmission of monetary policy shocks to mortgage rates in Germany and Switzerland. In Germany, fixed-rate mortgage rates respond more strongly to unexpected rate increases. In Switzerland, policy rates and fixed-rate mortgage rates instead co-move more strongly for rate cuts in the considered sample period.

In Appendix 2.10.2 we check the robustness of our results if we consider only monetary policy shocks that are negatively correlated with changes in stock market valuations, as measured by the Euro-Stoxx 50 and SMI, respectively.[27] Jarociński and Karadi (2020) have argued that this allows to separate policy rate shocks from news shocks that may be associated with monetary policy announcements. Hence, we require that an accommodative shock, for example, unexpectedly lowers interest rates and simultaneously increases the stock market valuation as predicted by standard asset pricing theory. If such a shock is instead associated with a decrease in the stock market valuation, we take this as a sign that the monetary policy announcement revealed also news about a worse economic outlook.

Distinguishing interest rate shocks from news shocks in this way, the results in Table 2.26 in Appendix 2.10.2, show that both interest rate and news shocks matter for housing tenure transitions. The effect on housing transitions tends to become stronger in Germany and Switzerland for the interest rate shocks relative to our benchmark results but the overall pattern of the estimates remains unchanged. Larger estimates are consistent with the interpretation that some news shocks offset part of the effect of interest rate shocks on housing tenure transitions. Table 2.26 in Appendix 2.10.2 further reveals that the response to news shocks is quantitatively the strongest in Switzerland.

[27] The correlation coefficient of the daily returns of the DAX and the MIB, respectively, with the daily returns of the Euro-Stoxx 50 is higher than 0.9, regardless of whether we use all daily returns in the sample period or only those on the announcement dates. Thus, we do not distinguish the stock-market index for Germany and Italy when constructing the shock series.

2.7 The response of rents and house prices

Interest rate shocks affect both housing tenure decisions and prices of housing units. In this section we estimate the effects of policy rate shocks on rental expenditures and housing values for Germany, Italy and Switzerland. Information on the value of housing is available at the household level for Germany and Italy but not for Switzerland, as further explained in the data appendix 2.10.5. Thus, we complement the evidence based on household-level data for Germany and Italy with evidence based on aggregate data for Switzerland.

Table 2.10 shows the effect of monetary policy shocks on rents (columns 1 to 3) and housing values (columns 4 to 6). The bottom row shows that an unexpected decrease of the interest rate reduces rents (but for Germany) and increases house prices.[28] Standard asset pricing theory tells us that the fundamental value of housing is determined by the present discounted value of rents.[29] If rents decrease, house prices may increase if the pass-through to interest rates, used for discounting the rents, is strong in the aftermath of the policy rate shocks, as documented in Section 2.4. Thus, the response of the fundamental value of housing depends on the size and persistence of the response of rents relative to interest rates.

In standard monetary transmission models with housing surveyed by Piazzesi and Schneider (2016), an unexpected cut of the interest rate is expansionary and increases consumption of housing and non-housing goods. Hence, one would expect that rents and house prices move in tandem. Our evidence, that rents decrease in Switzerland and also somewhat in Italy whereas the value of housing increases in both countries, suggests that borrowing constraints and market segmentation let prices of rented and owned units respond differently. Using aggregate data,

[28] We have investigated whether the changes in the values indeed reflect price effects and not changes in quantities. We have run the regressions on subsamples in the household data for which we have information for Germany and Italy on whether households have moved between survey waves. The results are not reported for brevity. We have found that the difference in the response of rents in Germany and Italy relative to Switzerland, which we report in Table 2.10, is robust if we consider only households that did not move between survey years. This suggests that price effects shape this difference.

[29] With an infinite horizon and stable rents and interest rates, the fundamental house value simplifies to the ratio of the rent to the interest rate.

Table 2.10: *Effect of monetary policy shocks on rents and house prices*

	Rents			Housing value / House price		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland†
Monetary policy shock, sum Q(-1,-4)	-0.054	-0.068	0.090***	0.022	-0.252**	-0.021
Monetary policy shock, sum Q(-5,-8)	-0.075	0.229	0.049**	-0.114	0.162	-0.024**
Monetary policy shock, sum Q(-9,-12)	-0.117*	-0.006	-0.000	-0.150**	-0.098	-0.009
Age controls	Yes	Yes	Yes	Yes	Yes	-
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	70,189	12,152	22,918	4,135	42,953	60
Adjusted R^2	0.01	0.02	0.02	0.02	0.03	0.99
Cum. effect (in percent) of 25 bp cut	6.15*	-3.88	-3.50***	6.04	4.71	1.36*

Notes: † Aggregate, quarterly data for Switzerland because no available household-level data. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in columns 1 to 3 is the log of real annual rent expenditures converted to adult-equivalent units using the equivalent scale detailed in Appendix 2.10.5. The dependent variable in columns 4 and 5 is the log of the deflated house price (2010 euros). Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The dependent variable in column 6 is the price index for flats in Switzerland retrieved from the BIS (Property prices statistics, reference *Q:CH:0:8:0:2:0:0*). The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

Corsetti et al. (2021) and Dias and Duarte (2019) show that the opposite response of rents and house prices to interest rate shocks also holds for the euro area and the U.S., respectively.[30]

Regarding house prices, columns 4 to 6 of Table 2.10 show that the effects of the policy rate shocks have a different timing across the three countries. The point estimates for the effect of a 25 bp cut are large, between 1.4 and 6 percent. Corsetti et al. (2021), Figure 8, report responses based on aggregate data for Germany and Italy that are an order of magnitude smaller. We also find smaller coefficients, in regressions not reported for brevity, if we use aggregate house price indexes for Germany and Italy. Hence, we do not interpret the cross-country differences in the size of the estimated effects because the underlying data series are differently measured. For Germany and Italy the estimates are based on household-level survey data (Appendix 2.10.5 describes the house price measures). For Switzerland, no such data are available so that the estimates in column 6 are based on an aggregate, quarterly price index for flats. Keeping in mind these caveats, it is still interesting to note that the strong increase of house prices in Italy in the year after a policy rate cut, displayed in column 5 of Table 2.10, is associated with the pattern in the response of the transition from renting to owning, which we reported in column 2 of Table 2.7 in Section 2.6. The coefficients of this response change sign for different lags of the shocks, suggesting that price effects may shape the pattern of the response over time by offsetting the immediate effect on the transitions resulting from the quick pass-through of the policy rate shocks to mortgage interest rates documented in subsection 2.4.2.

For rents, the data series are comparable across countries. Columns 1 to 3 of Table 2.10 show that rents respond strongest in Switzerland within the first year after the shock and the pass-through occurs within two years. The pass-through within the first year after the shock is weaker in Germany and Italy, and the pass-through across all lags of the shock series is much less precisely estimated in these countries. In the bottom row of Table 2.10 we report that an unexpected 25 bp cut of the policy rate reduces rents by 3.5 percent in Switzerland. The effect

[30] Given that these papers consider the response of nominal rents, it is worth noting that the results on the response of rents reported in Table 2.10 are quantitatively very similar if we use nominal rents as dependent variable.

for the other two countries is less clear. The point estimate for Italy is of similar size but not estimated precisely enough to be significant at the 10% level. For Germany, rents seem to increase rather than decrease after a policy rate cut but this effect is very noisily estimated. In Appendix 2.10.3 we check robustness of the results reported in Table 2.10 if we choose different time windows for constructing the policy rate shocks as in Corsetti et al. (2021) for the ECB and SNB as well as in Nakamura and Steinsson (2018a) for the SNB.

One possible reason for the strong and precisely estimated pass-through of policy rate shocks to rents in Switzerland is that rents are indexed to mortgage rates in Switzerland (see Section 2.2) and that the pass-through of the policy rate shocks to the mortgage rates is strongest in Switzerland (see Table 2.3 in subsection 2.4.2). To understand further what may cause the different responses of rents across countries, we investigate the response of rents by landlord type. We do this for Germany because the SOEP provides information on the landlord type since 2013.[31]

Table 2.27 in Appendix 2.10.3 shows that an unexpected cut of the policy rate in Germany reduces rents differently depending on the ownership type. We find that the pass-through to rents of publicly owned housing seems to differ from housing units with other ownership types. As mentioned in Section 2.2, publicly owned housing accounts for one third of the rental housing stock in Germany, for one fifth in Italy and only for one tenth in Switzerland. The results in Table 2.27 suggest that this may explain part of the differences in the pass-through to rents between Germany and Switzerland.

Further robustness checks presented in greater detail in Appendix 2.10.3 show that the findings reported in Table 2.10 are robust to using different time windows and shocks to yields of five-year government bonds. Only the response of rents in Italy then changes sign, but remains imprecisely estimated.

Together with the evidence on housing tenure transitions presented in Section 2.6, our findings suggests that the stronger pass-through to mortgage rates in

[31] Information on landlord type is not available in the SHP in Switzerland. In the SHIW for Italy, the sample size of renters is too small to provide insights by distinguishing the ownership type of rental units.

Switzerland relative to Germany and Italy triggered more transitions to homeownership within one year after an unexpected interest rate cut and an increase in the relative price of owning.

2.8 The transmission across Italian regions

In subsection 2.4.2 we uncovered heterogeneity across Italian regions with different financial development in terms of the pass-through of the policy rate shocks to mortgage interest rates. Also the incidence of mortgages is different: 17.5% in financially developed regions compared with 14.2% in less developed regions. We now exploit this heterogeneity to estimate the monetary transmission to the housing market based on an alternative identification. The estimated effects are identified by region-year variation. The interpretation of the coefficients is different to the aggregate effects estimated in our analysis so far because the effects are measured relative to a benchmark region rather than in absolute terms. The common intercept across regions included in the aggregate effect is differenced out. The approach of identifying the effects of macroeconomic policy using regional variation is similar to the estimation of relative fiscal multipliers by Nakamura and Steinsson (2014) for U.S. regions.

Table 2.11 displays the effect of monetary policy shocks on housing tenure transitions (columns 1 and 2), rents (column 3), house prices (column 4) and mortgage rates (column 5) in financially more developed Italian regions relative to the less developed regions. The results in column 5 confirm our finding from subsection 2.4.2 that the pass-through to mortgage rates is stronger in financially more developed regions. Column 5 shows results for households with variable-rate mortgage contracts in the SHIW whereas in subsection 2.4.2 we provided results for fixed-rate mortgage rates of new mortgage contracts.[32] Table 2.11 shows that the stronger pass-through to mortgage rates is associated with more transitions from

[32] Results, which are not reported for brevity, show that the pass-through to fixed-rate mortgage contracts of existing mortgagors is not different across regions. Adjustments of the contract rates for mortgages originated prior to the shocks are less relevant for the transition from renting to owning.

Table 2.11: *Baseline regression results for regional effects in Italy*

	Renter to owner	Owner to renter	Rents	House prices	Mortgage rate
Monetary policy shock, sum Q(-1:-4) × Developed	-0.061***	-0.004	-0.081**	0.006	0.449**
Monetary policy shock, sum Q(-5:-8) × Developed	0.066	-0.005	0.382***	0.154	0.646
Monetary policy shock, sum Q(-9:-12) × Developed	-0.024	-0.005	0.241***	0.127	0.114
Developed dummy	-0.026***	-0.002	0.525***	0.196***	-0.367***
Age controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Observations	5,645	22,142	12,085	42,765	2,493
Adjusted R^2	0.01	0.00	0.12	0.06	0.18
Cum. relative effect of 25 bp cut	0.23 pp	0.17 pp	-13.54*** percent	-7.17 percent	-0.30* pp

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not, in the tenure transition regressions. Log real annual rent expenditures are normalized by household size using the equivalent scale detailed in data appendix 2.10.5, the log real house price and log real rent expenditures are in units of euro in 2010, and the average variable mortgage rate is in percent. Standard errors are clustered by year. Age controls include age and age squared and refer to the household's reference person. The cumulative relative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative relative effects for Italy based on biannual transitions are adjusted to be comparable with those based on annual transitions.

renting to owning, a stronger reduction in rents, and no significantly different house price reactions in financially more developed regions than in less developed regions. Interestingly, the stronger pass-through to rents in financially more developed regions is associated with lower public ownership of rental housing (18% compared with 24%, based on information in the SHIW available for 2014 and 2016).

These results show that the effect of the monetary policy shocks on housing tenure transitions in the financially more developed Italian regions *relative* to the less developed regions is more similar to the aggregate effects on housing tenure which we estimated for Germany and Switzerland. This finding suggests that regional differences in the transmission of monetary policy shocks to mortgage interest rates and the user cost of homeownership help to explain regional differences in housing tenure transitions in Italy although the aggregate effect of housing tenure transitions in Italy seems to be explained less well by these determinants.

Thus, the results based on regional variation within Italy qualitatively tell a similar story as the evidence across countries. Differences in the pass-through of monetary policy to mortgage rates, associated with a different incidence of mortgages, imply substantial heterogeneity in the transmission of monetary policy to the housing market. In Appendix 2.10.4, we show that the results based on the regional variation are robust if we use different time windows for constructing monetary policy shocks as in Corsetti et al. (2021), if we use changes of long-term bond yields on the announcement dates as measures of the shocks, and if we vary the cut-off for classifying Italian regions as financially developed.

2.9 Conclusion

We have shown that the transmission of monetary policy to the housing market differs substantially across Germany, Italy, and Switzerland, and across regions within Italy. We have found differences in the pass-through of monetary policy shocks to mortgage rates, housing tenure transitions, rents and house prices. Our analysis indicates that differences in the incidence of mortgagors and renters; institutional features such as indexing rents to mortgage rates; and the extent of public housing shape the monetary policy transmission to the housing market.

From an applied theoretical point of view, our results help to discipline structural models that analyze monetary policy considering features of the housing market. The rich heterogeneity, that we have started to uncover in our analysis, hopefully will motivate additional empirical analysis. For the conduct of monetary policy, understanding the causes of the different pass-throughs to mortgage interest rates across regions, that we have documented, seems an important step for further research.

2.10 Appendix of chapter 2

2.10.1 Further evidence on interest rate shocks

Table 2.12: *Properties of cumulated ECB interest rate shocks*

	Raw	Cumulated by		
	series	Quarter	Semester	Year
Mean	0.001	0.002	0.004	0.008
Median	0.000	-0.003	0.013	-0.000
Standard deviation	0.072	0.127	0.162	0.208
Min	-0.225	-0.465	-0.650	-0.615
Max	0.280	0.420	0.335	0.335
Observations	229	72	36	18

Table 2.13: *Properties of cumulated SNB interest rate shocks*

	Raw	Cumulated by		
	series	Quarter	Semester	Year
Mean	-0.028	-0.034	-0.068	-0.136
Median	-0.010	-0.010	-0.030	-0.060
Standard deviation	0.099	0.141	0.170	0.247
Min	-0.510	-0.860	-0.780	-0.900
Max	0.170	0.310	0.230	0.160
Observations	87	72	36	18

Tables 2.12 and 2.13 show that the properties of the raw shock series are retained in the series with cumulated shocks. The mean and standard deviation become larger if the shocks are cumulated over longer periods, as documented in table 1 of Wong (2021) for the U.S.

We check whether the constructed monetary policy shocks are true shocks and thus not predictable by past values of the shocks. As mentioned in Section 2.3, private information of the monetary policy maker may introduce some persistence in our constructed series of shocks. Table 2.14 reports results of regressions of the monetary policy shocks on their lagged values. Columns 1 and 3 show results for regressions of the current quarterly shock on its past values for the ECB and the SNB, respectively. Columns 2 and 4 show regression results for the cumulated shock series where we check whether future shocks can be predicted by past values at different horizons. As mentioned in the main text in Section 2.3, the results by and large support that our constructed series of the shocks are not predictable by past values and thus are true shocks.

In our main regression specifications, we cumulate shocks for every year. Figure 2.5 shows that for these moving sums of the shocks, the autocorrelations are not significant beyond two quarters. Hence, multicollinearity of the lagged shocks in the regressions is not a concern.

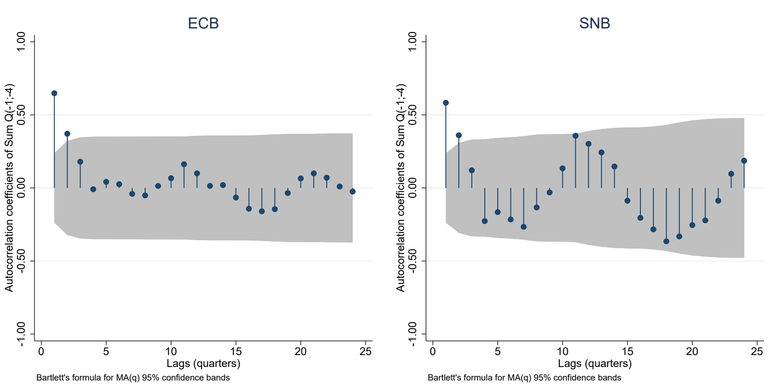
In Figure 2.6 we provide evidence that the three-month Euribor is highly correlated with the midpoint of the ECB policy rates, the rate on the main refinancing operations.

Table 2.15 shows that the pass-through of policy rate shocks to mortgage interest rates is stronger for Italy if we restrict the sample to shocks that are positively correlated with long-term yields. As mentioned in subsection 2.4.2 in the main text, this suggests that the pass-through to mortgage rates has been weaker in Italy during the euro-area debt crisis.

Table 2.14: *Regressions of current and future monetary policy shocks on past shocks*

	(1) ECB, Current quarterly shock	(2) ECB, Summed shocks in Q(+1,+4)	(3) SNB, Current quarterly shock	(4) SNB, Summed shocks in Q(+1,+4)
Monetary policy shock, Q(-1)	-0.146 (-1.16)		-0.190 (-1.52)	
Monetary policy shock, Q(-2)	-0.128 (-1.01)		-0.072 (-0.56)	
Monetary policy shock, Q(-3)	-0.053 (-0.45)		0.008 (0.06)	
Monetary policy shock, Q(-4)	0.068 (0.59)		-0.104 (-0.87)	
Monetary policy shock, sum Q(-1,-4)		0.025 (0.18)		-0.170 (-1.23)
Monetary policy shock, sum Q(-5,-8)		0.0101 (0.08)		0.028 (0.19)
Monetary policy shock, sum Q(-9,-12)		0.014 (0.11)		0.275* (2.00)
Observations	68	56	68	56
R^2	0.04	0.00	0.05	0.12

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors in brackets. Dependent variables are indicated at the top of the respective columns. All regressions include a constant. *Current shock* refers to the sum of the shocks that take place in a given quarter. *Sum Q(+1,+4)* denotes shocks cumulated over the next four quarters.



Notes: Correlograms of the moving sum of the quarterly shocks cumulated over a year.

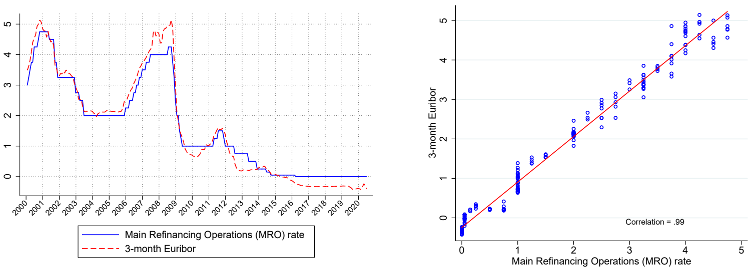
Figure 2.5: *Correlograms of the cumulated shock series*

Table 2.15: *Pass-through of policy rate shocks to five-year fixed-rate mortgage rates using shocks positively correlated with long-term yields*

	Germany	Italy	Switzerland
Monetary policy shocks, sum M(0)	0.012	-0.073	0.844^{***} †
Monetary policy shocks, sum M(-1)	0.288^{***}	0.361^{*}	-0.012
Monetary policy shocks, sum M(-2)	0.232^{**}	0.411^{*}	0.046
Cum. effect of 25 bp cut	-0.133^{***}	-0.175^{*}	-0.219^{***}
Monetary policy shocks, sum M(0)	0.005	-0.052	0.845^{***} †
Monetary policy shocks, sum M(-1)	0.267^{***}	0.396^{**}	-0.012
Monetary policy shocks, sum M(-2)	0.261^{**}	0.406	0.044
Monetary policy shocks, sum M(-3)	0.049	0.043	0.034
Cum. effect of 25 bp cut	-0.146^{***}	-0.199^{*}	-0.227^{***}

Sources: Rates of five-year fixed-rate mortgages from the ECB (Germany *MIR.M.DE.B.A2C.O.R.A.2250.EUR.N*, Italy *MIR.M.IT.B.A2C.O.R.A.2250.EUR.N*) and the SNB (*EPB@SNB.zikrepro{M,50}*) . *Notes:* Regression of monthly mortgage-rate changes on policy rate shocks cumulated by month. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The series are based on the monthly changes in the rates available for the period 2008M1-2017M12 in Switzerland and 2003M1-2017M12 in the euro area. The cumulative effect over three years of a -25 bp shock is computed by multiplying the sum of the coefficients by -0.25.

† Regular policy announcements at the SNB take place once a quarter. For the months without an announcement the value of the shock is zero.



Sources: Euribor and MRO rates from Thomson Reuters (Y03728 and S245UE). We plot the values at the end of each month for both series.

Figure 2.6: *Euribor and the rate on main refinancing operations (MRO) of the ECB*

2.10.2 Robustness of response of housing tenure

Different time windows around the policy announcements

We check robustness of the effect of policy rate shocks on housing tenure transitions if we use different time windows around the policy announcement. As alternatives to our benchmark of daily time windows, we use shorter time windows around the announcements, i.e., the six-hour time window used by Corsetti et al. (2021) for constructing interest rate shocks for the ECB, and the 30-minute window used by Gertler and Karadi (2015) and Nakamura and Steinsson (2018a) for constructing interest rate shocks for the Federal Reserve. For a description of the timing of the policy announcements see Section 2.3.

Table 2.16 shows the correlation between shock series that use different time windows on the days of the monetary policy announcements of the ECB and SNB, respectively. We compute the correlations for the raw series of shocks (one observation per policy announcement), and for the series with the shocks cumulated to quarters, semesters or years. The table shows that our benchmark series for monetary policy shocks in Switzerland is highly correlated if we use the alternative time windows with correlation coefficients of 0.94 and 0.86 for the shock series based on the six-hour and the 30-minute time window, respectively. For the euro area, the correlation between our benchmark raw series of the shocks and the series based on the six-hour time window is 0.7. The correlation between our benchmark series and the series based on the 30-minute window is smaller at 0.34. This illustrates that the communication of monetary policy by the ECB requires longer time windows for constructing interest rate shocks because the very short time window of 30 minutes does not capture all the new information which is released.

We thus report robustness checks for the euro-area countries Germany and Italy using the series for monetary policy shocks based on a six-hour time window, as in Corsetti et al. (2021). For Switzerland, we report robustness results for both the shock series based on the six-hour and the 30-minute time window.

Table 2.16 further shows that cumulation of the monetary policy shocks reduces the correlation between the series based on alternative time windows, as shocks partially wash out when they are cumulated. This is less the case for Switzerland than for the euro area.

Tables 2.17 and 2.18 display the results for the effect of the policy shocks on housing tenure transitions, using the shocks constructed with the alternative time windows mentioned above.

Table 2.16: *Correlation of benchmark with series for interest rate shocks using different time windows*

	ECB		SNB	
Correlation of benchmark with series using time windows as in	Corsetti et al. (2021)	Nakamura and Steinsson (2018a)	Corsetti et al. (2021)	Nakamura and Steinsson (2018a)
Raw series	0.695***	0.337***	0.944***	0.857***
Cumulated by				
... quarter	0.673***	0.330***	0.949***	0.902***
... semester	0.434***	0.150	0.935***	0.825***
... year	0.268	0.251	0.935***	0.807***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Correlations of the benchmark with shock series using different time windows, for the respective raw series of shocks and when cumulated for various frequencies.

Table 2.17: *The effect of monetary policy shocks on housing tenure transitions, using the time window as in Corsetti et al. (2021) for constructing the shocks*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.013	0.453***	-0.061	-0.001	0.033**	-0.071***
Monetary policy shock, sum Q(-5,-8)	-0.009	-0.006	-0.048	-0.003	-0.008**	-0.065***
Monetary policy shock, sum Q(-9,-12)	-0.018	0.192**	-0.026	-0.004	0.036**	0.012
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	67,521	5,677	21,039	71,320	22,219	24,777
Adjusted R^2	0.00	0.01	0.01	0.00	0.00	0.01
Cum. effect (in pp) of 25 bp cut	0.99	-7.97***	3.37	0.19	-0.76**	3.08***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions.

Table 2.18: *The effect of monetary policy shocks on housing tenure transitions in Switzerland, time window as in Nakamura and Steinsson (2018a) for constructing the shocks*

	Renter to owner		Owner to renter	
	Baseline	Nakamura and Steinsson (2018a)	Baseline	Nakamura and Steinsson (2018a)
Monetary policy shock, sum Q(-1,-4)	-0.0322***	-0.0680**	-0.0172***	-0.0827***
	(-8.49)	(-2.03)	(-4.93)	(-3.33)
Monetary policy shock, sum Q(-5,-8)	-0.0165***	-0.0810*	-0.00389	-0.0252*
	(-4.07)	(-2.00)	(-1.34)	(-1.82)
Monetary policy shock, sum Q(-9,-12)	-0.00411	-0.00233	0.00253	0.00979
	(-1.54)	(-0.08)	(1.14)	(0.73)
Age	Yes	Yes	Yes	Yes
Age squared	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	21039	21039	24777	24777
Adjusted R^2	0.01	0.01	0.01	0.01
Cum. effect (in pp) of 25 bp cut	1.32***	3.78**	0.46**	2.45***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The effects are larger for the Nakamura and Steinsson (2018a)'s time window by construction as the series have a smaller variance than the baseline series.

Using shocks to long-term yields

Table 2.19: *The effect of monetary policy shocks on housing tenure transitions using five-year government bond yield changes on announcement dates*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
5y yield change on announcement date, sum Q(-1,-4)	-0.020	0.038	-0.058***	0.005	0.003	-0.023***
5y yield change on announcement date, sum Q(-5,-8)	-0.031**	-0.025	-0.027***	0.003	-0.001	-0.004
5y yield change on announcement date, sum Q(-9,-12)	-0.017	0.006	0.008	-0.000	0.004	0.008
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	67,431	5,677	21,039	71,251	22,219	24,777
Adjusted R^2	0.00	0.00	0.01	0.00	0.00	0.00
Cum. effect (in pp) of 25 bp cut	1.68**	-0.25	1.94***	-0.20	-0.07	0.48

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions.

Excluding financial crisis years

Table 2.20: *The effect of monetary policy shocks on housing tenure transitions without financial crises years 2007/08*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.014	0.014	-0.033***	0.003	-0.012***	-0.019***
Monetary policy shock, sum Q(-5,-8)	-0.027	0.058*	-0.017***	0.001	0.010***	-0.005
Monetary policy shock, sum Q(-9,-12)	-0.024*	0.063***	-0.005*	0.001	0.009***	0.003
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	59,106	4,894	18,550	61,698	19,293	21,932
Adjusted R^2	0.00	0.01	0.01	0.00	0.00	0.01
Cum. effect (in pp) of 25 bp cut	1.62*	-1.69***	1.38***	-0.14	-0.08**	0.51***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions.

Excluding periods with negative interest rate policies

Table 2.21: *The effect of monetary policy shocks on housing tenure transitions excluding quarters with a negative interest rate policy (NIRP)*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.017	-0.020**	-0.032***	-0.002	-0.003***	-0.017***
Monetary policy shock, sum Q(-5,-8)	-0.029*	0.040**	-0.016***	-0.000	0.016***	-0.003
Monetary policy shock, sum Q(-9,-12)	-0.024*	0.061***	-0.004	0.004	0.012***	0.003
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	55,410	4,311	16,465	59,578	16,776	19,096
Adjusted R^2	0.00	0.01	0.01	0.00	0.00	0.01
Cum. effect (in pp) of 25 bp cut	1.76**	-1.01***	1.29***	-0.06	-0.32***	0.44**

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions. The regressions exclude periods from Q3, 2014 onward for the euro area (as the ECB deposit facility rate became negative in Q2, 2014) and from Q1, 2015 onward for Switzerland (as the SNB sight deposit rate became negative in Q4, 2014).

Additional lags

Table 2.22: *The effect of monetary policy shocks on housing tenure transitions with additional lags*

	Renter to owner						Owner to renter					
	Germany		Italy		Switzerland		Germany		Italy		Switzerland	
Monetary policy shock, sum Q(-1,-4)	-0.017	-0.024	-0.021**	-0.024**	-0.035***	-0.043***	-0.001	0.003	-0.003	-0.003*	-0.015***	-0.012***
Monetary policy shock, sum Q(-5,-8)	-0.043**	-0.053***	0.062**	0.104***	-0.021***	-0.038***	0.001	0.005	0.008	0.011*	-0.001	0.005
Monetary policy shock, sum Q(-9,-12)	-0.034**	-0.042**	0.068***	0.070***	-0.004**	-0.034**	-0.001	0.005	0.007**	0.008***	0.001	0.011
Monetary policy shock, sum Q(-13,-16)		-0.012		0.062***		-0.028*		0.008		0.005*		0.009*
Monetary policy shock, sum Q(-17,-20)		-0.023				-0.033*		-0.004				0.013*
Monetary policy shock, sum Q(-21,-24)		-0.006				-0.050*		-0.006				0.014
Age controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Year dummies	Yes	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Linear trend	No	No	Yes	Yes	No	No	No	Yes	Yes	Yes	No	No
Observations	53,574	53,574	5,026	5,026	18,259	18,259	56,688	56,688	19,775	19,775	21,983	21,983
Adjusted R^2	0.00	0.00	0.01	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.01	0.01
Cum. effect (in pp) of 25 bp cut	2.35**	4.00**	-1.36**	-2.65***	1.51***	5.64**	0.03	-0.27	-0.15*	-0.26**	0.36**	-1.01

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows the results of tenure transition regressions with additional lags. For Italy, the available degrees of freedom do not allow to include more than one additional lag. For Germany and Switzerland, the available sample shortens starting with the year 2006. For Italy, the available sample shortens starting with the year 2003. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions.

Additional household controls

Table 2.23: *The effect of monetary policy shocks on housing tenure transitions with additional controls*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.015	-0.021***	-0.029***	0.002	-0.004	-0.018***
Monetary policy shock, sum Q(-5,-8)	-0.028**	0.051**	-0.013***	0.001	0.009	-0.008***
Monetary policy shock, sum Q(-9,-12)	-0.025**	0.068***	-0.002	0.003	0.008**	0.000
Household controls†	Yes	Yes	Yes	Yes	Yes	Yes
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	67,431	5,677	21,039	71,251	22,219	24,777
Adjusted R^2	0.02	0.02	0.02	0.01	0.02	0.02
Cum. effect (in pp) of 25 bp cut	1.70**	-1.21***	1.10***	-0.13	-0.17	0.66***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions. † Household controls include household size, employment status, civil status, relationship status, gender, domestic nationality and real household income. Household income is gross for Germany and Switterland and net for Italy.

Non-linear probability models for housing tenure

Table 2.24: *Different specifications for the effect of monetary policy shocks on housing tenure transitions*

	Renter to owner						Owner to renter					
	Probit			Logit			Probit			Logit		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.013	-0.024***	-0.027***	-0.014	-0.023***	-0.026***	0.003	-0.003**	-0.015***	0.002	-0.003**	-0.014***
Monetary policy shock, sum Q(-5,-8)	-0.022*	0.059**	-0.012***	-0.022*	0.056**	-0.011***	0.003	0.007	-0.002	0.002	0.007	-0.002
Monetary policy shock, sum Q(-9,-12)	-0.022*	0.070***	0.000	-0.021*	0.069***	0.001	0.003	0.007***	0.002	0.003	0.007***	0.003
Age controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No
Observations	67,431	5,677	21,039	67,431	5,677	21,039	71,251	22,219	24,777	71,251	22,219	24,777

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The reported coefficients are marginal effects (and are hence comparable across specifications), computed at the mean. For the computation of the marginal effects in the logit and probit specifications, we set the monetary policy shocks to zero - which is the approximate value of the shocks' mean.

Asymmetric effect of positive or negative interest rate shocks?

Table 2.25: *The effect of monetary policy shocks on housing tenure transitions distinguishing positive and negative shocks*

	Renter to owner			Owner to renter		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland
Monetary policy shock, sum Q(-1,-4)	-0.038**	0.047***	-0.066	0.014**	-0.003***	-0.073
Monetary policy shock, sum Q(-5,-8)	-0.038*	0.198***	-0.172	-0.003	0.006***	0.121
Monetary policy shock, sum Q(-9,-12)	-0.048**	0.092***	0.039	0.008	0.025***	-0.056
Monetary policy shock, sum Q(-1,-4) × Negative shock	0.036	-0.088***	0.037	-0.030***	-0.001	0.057
Monetary policy shock, sum Q(-5,-8) × Negative shock	0.020	-0.240***	0.161	-0.001	-0.013***	-0.130
Monetary policy shock, sum Q(-9,-12) × Negative shock	0.054**	-0.001	-0.049	-0.021**	-0.023***	0.065
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	67,431	5,677	21,039	71,251	22,219	24,777
Adjusted R^2	0.00	0.01	0.01	0.00	0.00	0.00
Cum. effect (in pp) of 25 bp decrease	0.30	-0.11***	1.23***	0.82*	0.12***	0.38
Cum. effect (in pp) of 25 bp increase	-3.03**	4.23***	-4.96	0.48	0.35***	-0.18

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions. Note that the cumulative effect of a 25 bp change can be significant although the individual coefficients are not significant because of the covariance of the estimates.

Distinguishing types of monetary policy shocks

Table 2.26: *The effect of monetary policy shocks when conditioning the shocks on stock market movements*

	Renter to owner						Owner to renter					
	Germany		Italy		Switzerland		Germany		Italy		Switzerland	
	Interest	News	Interest	News	Interest	News	Interest	News	Interest	News	Interest	News
Monetary policy shock, sum Q(-1,-4)	-0.057**	0.000	0.059*	-0.012*	-0.037***	-0.101***	-0.010	0.009	0.002	-0.001	-0.025***	-0.043***
Monetary policy shock, sum Q(-5,-8)	0.010	-0.022	0.007	0.000	-0.019*	-0.053***	-0.034***	0.016**	-0.003	0.001	-0.007	-0.006
Monetary policy shock, sum Q(-9,-12)	-0.050**	-0.010	-0.047	0.030	-0.002	-0.021**	-0.008	0.008	0.001	0.002***	0.007	-0.004
Age controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Year dummies	Yes	Yes	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Linear trend	No	No	Yes	Yes	No	No	No	Yes	Yes	Yes	No	No
Observations	67,431	67,431	5,677	5,677	21,039	21,039	71,251	71,251	22,219	22,219	24,777	24,777
Adjusted R^2	0.00	0.00	0.00	0.00	0.01	0.01	0.00	0.00	0.00	0.00	0.01	0.01
Cum. effect (in pp) of 25 bp cut	2.45*	0.78	-0.25	-0.23	1.47***	4.38***	1.29***	-0.82**	0.01	-0.03	0.63*	1.33***

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows the results of separate regressions using either interest shocks or news shocks. Interest shocks are all baseline monetary policy shocks that have the opposite sign as the return of respective the stock market index (EURO STOXX 50/SMI) on the announcement date. The remaining baseline shocks are called news shocks. The dependent variable is 1 if a household changed tenure status and 0 if it does not. Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those for Germany and Switzerland based on annual transitions.

2.10.3 Robustness of response of rents and house prices

Table 2.27: *The effect of monetary policy shocks on rents: Germany, by landlord type*

	(1) All landlord types All years	(2) All landlord types Years from 2013	(3) Private companies Years from 2013	(4) Private owner Years from 2013	(5) Cooperative Years from 2013	(6) Public Years from 2013
Monetary policy shock, sum Q(-1,-4)	-0.0542	0.271	0.451	0.370	1.066**	-0.582
	(-1.14)	(0.84)	(0.80)	(0.99)	(2.83)	(-0.69)
Monetary policy shock, sum Q(-5,-8)	-0.0747	0.233	0.412	0.231	0.594*	-0.0637
	(-1.06)	(0.84)	(0.87)	(0.72)	(1.87)	(-0.09)
Monetary policy shock, sum Q(-9,-12)	-0.117*	-0.0102	-0.146	-0.0184	0.194	-0.168
	(-1.74)	(-0.08)	(-0.71)	(-0.13)	(1.44)	(-0.56)
Age	Yes	Yes	Yes	Yes	Yes	Yes
Age squared	Yes	Yes	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70189	21138	2099	12309	3942	1915
Adjusted R^2	0.01	0.02	0.04	0.03	0.04	0.03
Cum. effect (in percent) of 25 bp cut	6.15*	-12.35	-17.92	-14.56	-46.37**	20.34

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log of real annual rent expenditures converted to adult-equivalent units using the equivalent scale detailed in Appendix 2.10.5. Standard errors are clustered by quarter of the interview because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

Column 2 of Table 2.27 shows that a cut of the interest rate reduces rents in Germany in the sample period since 2013, in which the SOEP contains information the landlord type. Hence, the pass-through has become qualitatively more similar to Switzerland compared to our benchmark period (see Table 2.10, column 3). Some of the effects reported in Table 2.27 are large. The effects in columns 3, 4 and 5 would be smaller (in absolute terms), and they are thus less robust than the effect on rents for public housing in column 6, if we used shocks to yields of five-year government bonds in the regression instead of the interest rate shocks. Column 6 of Table 2.27 shows that the pass-through to rents seems to differ for public housing.

We check robustness of the results reported in Table 2.10 if we use the time windows for constructing monetary policy shocks as in Corsetti et al. (2021) or Nakamura and Steinsson (2018a). The net effect on rents for Italy, reported in the bottom row of Table 2.28, column 2, changes sign but is still imprecisely estimated. Otherwise, the results in Table 2.28 and Table 2.29 are similar to our benchmark results reported in Table 2.10, Section 2.7.

Finally, Table 2.30 shows that the results, reported in Table 2.10 in the main text, are robust if we use shocks to the yields of five-year government bonds instead of policy rate shocks. The exception is again the effect on rents in Italy which changes sign but it is not precisely estimated.

Table 2.28: *The effect of monetary policy shocks on rents and house prices, using the time window as in Corsetti et al. (2021) for constructing the shocks*

	Rents			Housing value / House price		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland†
Monetary policy shock, sum Q(-1,-4)	-0.094**	-0.336	0.136***	0.032	-0.993	-0.024
Monetary policy shock, sum Q(-5,-8)	-0.069	-0.200*	0.064**	-0.048	-0.803***	-0.027
Monetary policy shock, sum Q(-9,-12)	-0.128	0.223	0.012	-0.228***	0.812	-0.003
Age controls	Yes	Yes	Yes	Yes	Yes	-
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	70,189	12,152	22,918	4,135	42,953	60
Adjusted R^2	0.01	0.02	0.02	0.02	0.04	1.00
Cum. effect (in percent) of 25 bp cut	7.30	7.83	-4.69**	6.10	24.58	1.36

Notes: † Aggregate, quarterly data for Switzerland because no available household-level data. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in columns 1 to 3 is the log of real annual rent expenditures converted to adult-equivalent units using the equivalent scale detailed in Appendix 2.10.5. The dependent variable in columns 4 and 5 is the log of the deflated house price (2010 euros). The dependent variable in column 6 is the price index for flats in Switzerland retrieved from the BIS (Property prices statistics, reference *Q:CH:0:8:0:2:0:0*). Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

Table 2.29: *The effect of monetary policy shocks on rents and house prices in Switzerland, using the time window as in Nakamura and Steinsson (2018a) for constructing the shocks*

	Rent		House price†	
	Baseline	Nakamura and Steinsson (2018a)	Baseline	Nakamura and Steinsson (2018a)
Monetary policy shock, sum Q(-1,-4)	0.090***	0.126***	-0.021	-0.010
Monetary policy shock, sum Q(-5,-8)	0.050**	0.037	-0.024**	-0.015
Monetary policy shock, sum Q(-9,-12)	-0.000	-0.022	-0.009	0.001
Age	Yes	Yes	-	-
Age squared	Yes	Yes	-	-
Quarter dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	22,918	22,918	60	60
Adjusted R^2	0.02	0.02	0.99	1.00
Cum. effect (in percent) of 25 bp cut	-3.50***	-3.51**	1.36*	0.60

Notes: † Aggregate, quarterly data for Switzerland because no available household-level data on house prices. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in columns 1 and 2 is the log of real annual rent expenditures converted to adult-equivalent units using the equivalent scale detailed in Appendix 2.10.5. The dependent variable in columns 3 and 4 is the price index for flats in Switzerland retrieved from the BIS (Property prices statistics, reference *Q:CH:0:8:0:2:0:0*). Standard errors are clustered by quarter of the interview because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

Table 2.30: *The effect of monetary policy shocks on rents and house prices, using five-year bond yield changes on announcement dates*

	Rents			Housing value / House price		
	Germany	Italy	Switzerland	Germany	Italy	Switzerland†
5y yield change on announcement date, sum Q(-1,-4)	-0.083	-0.005	0.149***	0.022	-0.455***	-0.045*
5y yield change on announcement date, sum Q(-5,-8)	-0.053	0.032	0.067**	-0.034	0.209**	-0.051**
5y yield change on announcement date, sum Q(-9,-12)	-0.074	-0.164*	-0.000	-0.036	-0.027	-0.032**
Age controls	Yes	Yes	Yes	Yes	Yes	-
Quarter dummies	Yes	No	Yes	Yes	No	Yes
Year dummies	Yes	No	Yes	Yes	No	Yes
Linear trend	No	Yes	No	No	Yes	No
Observations	70,189	12,152	22'918	4,135	42,953	60
Adjusted R^2	0.01	0.02	0.02	0.02	0.03	1.00
Cum. effect (in percent) of 25 bp cut	5.23	3.42	-5.39***	1.19	6.80	3.21**

Notes: † Aggregate, quarterly data for Switzerland because no available household-level data. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in columns 1 to 3 is the log of real annual rent expenditures converted to adult-equivalent units using the equivalent scale detailed in Appendix 2.10.5. The dependent variable in columns 4 and 5 is the log of the deflated house price (2010 euros). The dependent variable in column 6 is the price index for flats in Switzerland retrieved from the BIS (Property prices statistics, reference *Q:CH:0:8:0:2:0:0*). Standard errors are clustered by quarter of the interview for Germany and Switzerland and by year for Italy, because the monetary policy shock does not vary at the household level. Age controls include age and age squared and refer to the household's reference person. The cumulative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25.

2.10.4 Robustness of results across Italian regions

We show that the results reported in the main text are robust if we use different time windows for constructing monetary policy shocks as in Corsetti et al. (2021), and if we vary the cut-off for classifying Italian regions as financially developed. In Table 2.34 we find that the coefficient of the interest rate shocks, with a lag between one and two years, increases for the transition from rental to owning if we add Umbria to the group of less developed regions. This changes the net effect of the shocks over all lags reported in the bottom row. The immediate effect within a year, which is most precisely estimated, remains quantitatively similar however. See Table 2.37 in the data appendix 2.10.5 for the classification of Italian regions in terms of their financial development based on the indicator provided by Guiso et al. (2004).

Table 2.31: *Regression results for regional effects in Italy, using the time window as in Corsetti et al. (2021) for constructing the shocks*

	Renter to owner	Owner to renter	Rents	House prices	Mortgage rate
Monetary policy shock, sum Q(-1:-4) × Developed	-0.406***	0.013	0.373	0.291**	-0.296
Monetary policy shock, sum Q(-5:-8) × Developed	-0.100**	-0.014	-0.028	0.241***	0.099
Monetary policy shock, sum Q(-9:-12) × Developed	0.363***	-0.070**	0.654*	0.496***	0.891
Developed dummy	-0.029***	0.001	0.476***	0.160***	-0.432**
Age controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Observations	5,645	22,142	12,085	42,765	2,493
Adjusted R^2	0.01	0.00	0.12	0.06	0.18
Cum. relative effect of 25 bp cut	1.78* pp	0.88** pp	-24.97*** percent	-25.71*** percent	-0.17 pp

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not for the tenure transition regressions. Real annual rent expenditures are logs of rents in 2010 euro converted into adult equivalents for each household, real house price are logs of prices in 2010 euro, and the variable-rate mortgage rate is in percent. Standard errors are clustered by year. Age controls include age and age squared and refer to the household's reference person. The cumulative relative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those based on annual transitions.

Table 2.32: *Regression results for regional effects in Italy using 5-year bond yield changes on announcement dates*

	Renter to owner	Owner to renter	Rents	House prices	Mortgage rate
5y yield change on announcement date, sum Q(-1:-4) × Developed	-0.138***	-0.002	0.124	0.122	0.963*
5y yield change on announcement date, sum Q(-5:-8) × Developed	0.042**	0.005	-0.134**	-0.084	-0.563***
5y yield change on announcement date, sum Q(-9:-12) × Developed	-0.047	-0.009	0.061	-0.025	0.764
Developed dummy	-0.031***	-0.003	0.542***	0.203***	-0.329***
Age controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Observations	5,645	22,142	12,085	42,765	2,493
Adjusted R^2	0.01	0.00	0.12	0.06	0.18
Cum. relative effect of 25 bp cut	1.78** pp	0.08 pp	-1.26 percent	-0.33 percent	-0.29 pp

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not for the tenure transition regressions. Real annual rent expenditures are logs of rents in 2010 euro converted into adult equivalents for each household, real house price are logs of prices in 2010 euro, and the variable-rate mortgage rate is in percent. Standard errors are clustered by year. Age controls include age and age squared and refer to the household's reference person. The cumulative relative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those based on annual transitions.

Table 2.33: *Regression results for regional effects in Italy with Sardinia in developed group*

	Renter to owner	Owner to renter	Rents	House prices	Mortgage rate
Monetary policy shock, sum Q(-1:-4) × Developed	-0.034**	-0.002	-0.103***	0.025	0.275
Monetary policy shock, sum Q(-5:-8) × Developed	0.017	-0.006	0.356***	0.143	0.977**
Monetary policy shock, sum Q(-9:-12) × Developed	-0.032	-0.006	0.245***	0.096	0.020
Developed dummy	-0.029***	-0.004*	0.461***	0.143***	-0.357***
Age controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Observations	5,645	22,142	12,085	42,765	2,493
Adjusted R^2	0.01	0.00	0.10	0.05	0.18
Cum. relative effect of 25 bp cut	0.61 pp	0.17 pp	-12.46*** percent	-6.58 percent	-0.32* pp

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not for the tenure transition regressions. Real annual rent expenditures are logs of rents in 2010 euro converted into adult equivalents for each household, real house price are logs of prices in 2010 euro, and the variable-rate mortgage rate is in percent. Standard errors are clustered by year. Age controls include age and age squared and refer to the household's reference person. The cumulative relative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those based on annual transitions.

Table 2.34: *Regression results for regional effects in Italy with Umbria in less developed group*

	Renter to owner	Owner to renter	Rents	House prices	Mortgage rate
Monetary policy shock, sum Q(-1:-4) × Developed	-0.068***	-0.004	-0.075**	0.034	0.395**
Monetary policy shock, sum Q(-5:-8) × Developed	0.093*	-0.002	0.368***	0.111	0.427
Monetary policy shock, sum Q(-9:-12) × Developed	-0.009	-0.003	0.206***	0.110	0.171
Developed dummy	-0.022***	-0.000	0.532***	0.200***	-0.400***
Age controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Observations	5,645	22,142	12,085	42,765	2,493
Adjusted R^2	0.01	0.00	0.12	0.06	0.18
Cum. effect of 25 bp cut	-0.19 pp	0.12 pp	-12.47*** percent	-6.37 percent	-0.25 pp

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is 1 if a household changed tenure status and 0 if it does not for the tenure transition regressions. Real annual rent expenditures are logs of rents in 2010 euro converted into adult equivalents for each household, real house price are logs of prices in 2010 euro, and the variable-rate mortgage rate is in percent. Standard errors are clustered by year. Age controls include age and age squared and refer to the household's reference person. The cumulative relative effect over three years of a -25 bp shock is obtained by multiplying the sum of the coefficients with -0.25. The reported cumulative effects for Italy based on biannual transitions are adjusted to be comparable with those based on annual transitions.

2.10.5 Data appendix

The German Socioeconomic Panel (SOEP), the Swiss Household Panel (SHP) and the Italian Survey on Household Income and Wealth (SHIW) are unbalanced household panels. Households are interviewed once a year in the SOEP and SHP and every other year in the SHIW. The SOEP contains information on households since 1990, the SHP since 1999, and the SHIW since 1977. For all three countries, we use the data on households in the time period 2000-2016 together with the data series which we constructed for the monetary policy shocks. The summary statistics are reported for the sample period 2002-2016, which is used in our estimations because monetary policy shocks enter with lags in the estimated specifications.

For the household-level data in all three countries, our constructed sample consists of households for which the following variables were recorded: housing tenure (renter or owner), age, household size (number of people), civil status (married or not as well as in a partnership or not), working status (yes or no), the interviewee's gender, nationality and region, and household income. The SOEP and

SHP provide information on household gross income, which includes labor earnings, capital income, pensions and (government) transfers. We keep households in the sample which report non-negative household income. In the SHIW, the reported net income includes transfers between households so that net income can be negative if households make a transfer. Thus, we keep the few households in the sample that report negative net income. We further keep households whose interviewee is the household head (or partner), and we focus on household heads with an age between 25 and 84. This covers ages at which most households have finished full-time education, have entered the labor market or are retired. The constructed sample contains $138,682$, $45,816$ and $27,896$ households, respectively, for the SOEP, the SHP and the SHIW in the sample period used for the estimations.

We now discuss for each country how we obtain information on the other variables used in the estimations. For Germany, data on rents, net worth and an approximated house price are available at the household-level in the SOEP. Households report rent expenditure (annualized and net of utility expenditure) and net worth (total wealth net of debt). We approximate house prices using the (annualized) mortgage expenditure reported in the data. This expenditure contains both amortization and interest payments, which cannot be distinguished in the data. We approximate house prices for households who moved into their current property in the previous two years because these households are less likely to have altered their initial mortgage contract, and the purchase price of their home reflects current economic conditions. We assume mortgage amortization over a typical 25-year period and a loan-to-value ratio of 50%, which corresponds to the average loan-to-value ratio of German mortgagors, aged 36-45 and thus with relatively new mortgages, in the 2014 wave of the Household Finance and Consumption Survey (HFCS). We further use the mortgage rate for the year in which the household moved in. Specifically, we use the annualized agreed mortgage rate for Germany provided by the ECB (key: *124.MIR.M.DE.B.A2C.AM.R.A.2250.EUR.N*), which is the agreed interest rate for new mortgages to the household sector.

We assume constant mortgage payments over time as in the typical mortgage contract observed in Germany.[33] Because mortgage payments consist of amortization and interest payments, the amortization payment A_t of a household, for a mortgage D_t in period t and a fixed mortgage interest rate i, is given by:

$$A_t = \left(\frac{1}{(1+i)^{t+1}-1}\right) D_t \ . \tag{2.1}$$

The mortgage changes over time due to amortization: $D_{t+1} = D_t - A_t$. The interest payment is iD_t, and the share of mortgage payments used for servicing the interest is $s_t = \frac{iD_t}{A_t+iD_t}$. The amortization rate is defined as $a_t = \frac{A_t}{D_t}$ so that $D_{t+1} = D_t(1-a_t)$.

We use the mortgage payments reported by a household in the SOEP, multiplied by the share s_t, to retrieve the interest payment at the household level, and we obtain the current mortgage value by dividing this interest payment by the mortgage interest rate. We then back out the initial mortgage value at purchase by adding the amortization payments to the current mortgage value. We divide the current mortgage value by $(1-a_t)$ for households who moved in last year, and by $(1-a_t)(1-a_{t-1})$ for households who moved in two years ago. Dividing the initial mortgage value by the loan-to-value ratio of 50%, we obtain the approximated house value. The average (median) approximated house value amounts to EUR 206,293 (178,620) and is thus in the ballpark of the values reported by households in the Household Finance and Consumption Survey (HFCS) for Germany in 2014. Given that we need information on house values before and after the monetary policy shocks, we cannot use the survey information on house values in the HFCS directly for our analysis because that survey has been conducted only every three years since 2010.

For Switzerland, we do not have household-level measures on house values and net worth. We also do not know which households are new homeowners in the Swiss household budget survey (*Haushaltsbudgeterhebung* or HABE), which contains information on mortgage expenditures, so that we cannot apply the approximation

[33] The estimation results based on the approximated house prices are unchanged if we assume linear amortization.

of house prices discussed for the SOEP above. Thus, as mentioned in the main text, we use aggregate-level house prices for Switzerland. For expenditures on housing rents, we use household-level data from the HABE. The HABE is a repeated cross-sectional data set. It contains data on detailed household income and expenditure items between 2000Q1 and 2017Q4 and is used for the national CPI calculations. We construct the sample for the HABE, proceeding analogously as before for the SOEP, SHP and SHIW, by keeping households whose interviewee is the reference person (the household head) and is aged between 25 and 84. We further keep households which report positive gross income, mandatory payments (which include taxes and mandatory health insurance), disposable income (income minus mandatory payments), consumption expenditure and rent. To contain the effect of potential outliers, we trim the sample by keeping households which report a gross saving rate (measured as disposable income minus expenditure, divided by disposable income) between the first and 99th percentiles in a given interview year. The final sample used for the estimation contains $22,918$ renters.

For Italy, the information on house prices is based on the self-reported house value provided by households in the SHIW. Specifically, households are asked: "In your opinion, how much is your house/flat worth (unoccupied)? In other words, what price could you ask for it today (including any cellar, garage or attic)? Please give your best estimate." For rents, we use reported annual rent payments. In order to identify households, who have not moved between survey years, we restrict the sample to those renters who provide the same construction year of their flat/house for consecutive survey years. For net worth, we use the definition provided in the SHIW, which is the value of real assets plus the value of financial assets minus the value of financial liabilities. The median net worth for each transition group is reported in Table 2.6, and the median net worth for each of these groups by region is shown in Table 2.38.

For all three considered countries, we convert the series for rental expenditures to adult-equivalent units by using the equivalent scale of Fernández-Villaverde and Krueger (2007) in Table 1, column 7 (p. 554). For households with more than five persons, the scale is increased by 0.3 per additional person which equals the increment for the fifth person in Fernández-Villaverde and Krueger (2007). When indicated, the series are deflated by using the annual nationwide CPI with 2010 as the base year. For Germany and Switzerland we use the series with the FRED codes *DEUCPIALLMINMEI* and *CHECPIALLMINMEI*, respectively. For Italy, we use the consumption deflator provided in the SHIW from Istat.

In Tables 2.35 to 2.39 we provide further descriptive evidence. Tables 2.35 and 2.36 show that some household characteristics are different across language groups in Switzerland, whether we classify language groups using information on both the reported mother tongue and the location of residence in Table 2.35, or using only information on the reported mother tongue in Table 2.36. For instance, Italian-speaking households have lower income across all housing tenure groups. Given these differences, we control for income as well as some other household characteristics, when we analyze the effect of interest rate shocks on housing tenure transitions across Swiss language groups in Section 2.6.

Table 2.37 shows how we classify Italian regions in terms of their financial development based on the indicator provided by Guiso et al. (2004). We use the binary classification to construct the spread in the mortgage rates between regions that are more and less financially developed. In Tables 2.33 and 2.34 in Appendix 2.10.2, we provide robustness checks if we assign the marginal regions, Umbria or Sardinia, to the respective other category.

Table 2.38 provides summary statistics for the subpopulations that change, or do not change, housing tenure status in the Italian regions with a different degree of financial development. Table 2.38 shows that the differences across the subpopulations discussed in the main text for Table 2.6, Section 2.5, also hold for the two types of Italian regions. The minor differences in the total sample size compared to Table 2.6 result from some households that we drop because they change region type.

Table 2.39 shows how key characteristics in the housing and mortgage market differ across Italian regions with different financial development.

Table 2.35: *Summary statistics for housing-tenure groups in Switzerland, across language groups*

	Swiss-German			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	14,447	615	18,779	301
Age (household head)	50.2	45.6	56.0	55.1
Household size (persons)	2.2	2.8	2.8	2.1
In a couple (%)	53.4	77.7	80.0	53.5
Married (%)	40.9	59.8	75.3	44.5
Working (%)	71.3	79.8	67.2	64.1
Gender (% male)	37.7	37.6	40.6	39.9
Domestic citizenship (%)	90.9	91.2	95.5	97.3
Gross household income (2010 CHF, annual)	94,068	128,563	122,211	98,664
	German			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	524	17	439	13
Age (household head)	50.8	44.8	57.0	62.5
Household size (persons)	2.1	2.6	2.7	1.6
In a couple (%)	55.5	58.8	80.2	53.8
Married (%)	38.5	58.8	75.9	30.8
Working (%)	71.4	82.4	61.5	30.8
Gender (% male)	39.1	29.4	44.6	30.8
Domestic citizenship (%)	57.8	76.5	66.7	92.3
Gross household income (2010 CHF, annual)	108,641	146,193	130,578	66,937
	French			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	4,459	151	4,116	75
Age (household head)	51.3	43.8	55.3	53.6
Household size (persons)	2.3	3.0	2.8	2.3
In a couple (%)	54.0	83.4	80.8	53.3
Married (%)	44.0	69.5	77.3	44.0
Working (%)	64.0	83.4	63.6	66.7
Gender (% male)	33.6	39.1	36.2	30.7
Domestic citizenship (%)	85.8	90.1	91.4	92.0
Gross household income (2010 CHF, annual)	94,070	132,157	124,230	103,226
	Italian			
	Renters		**Owners**	
	Remained renter	Became owner	Remained owner	Became renter
Observations	789	37	1,035	19
Age (household head)	54.3	44.9	55.6	58.5
Household size (persons)	2.2	3.0	2.7	1.7
In a couple (%)	53.1	70.3	72.2	36.8
Married (%)	48.3	56.8	70.0	31.6
Working (%)	51.0	70.3	53.5	47.4
Gender (% male)	36.9	29.7	35.6	47.4
Domestic citizenship (%)	70.5	75.7	81.0	84.2
Gross household income (2010 CHF, annual)	72,362	92,386	97,363	64,813

Source: SHP. *Notes:* Averages for households interviewed between 2002-2016. Medians for income measures. Changes in tenure refer to changes since the last survey. Real incomes are deflated by the national CPI. 19,474 out of 45,816 households report their main language. For the other households, we assign the language based on the location in which the household lives.

Table 2.36: *Summary statistics for housing-tenure groups in Switzerland, across groups of reported mother tongue*

	Renters		Owners	
Swiss-German				
	Remained renter	Became owner	Remained owner	Became renter
Observations	4'153	163	5'976	91
Age (household head)	52.9	46.3	56.4	57.3
Household size (persons)	2.1	2.7	2.7	2.1
In a couple (%)	52.8	79.1	78.8	50.6
Married (%)	44.6	62.6	74.5	49.5
Working (%)	69.1	76.1	67.6	54.9
Gender (% male)	39.5	39.3	45.1	42.9
Domestic citizenship (%)	95.5	95.7	98.2	100.0
Gross household income (2010 CHF, annual)	94'355	123'357	123'330	96'774
German				
	Remained renter	Became owner	Remained owner	Became renter
Observations	524	17	439	13
Age (household head)	50.8	44.8	57.0	62.5
Household size (persons)	2.1	2.6	2.7	1.6
In a couple (%)	55.5	58.8	80.2	53.8
Married (%)	38.5	58.8	75.9	30.8
Working (%)	71.4	82.4	61.5	30.8
Gender (% male)	39.1	29.4	44.6	30.8
Domestic citizenship (%)	57.8	76.5	66.7	92.3
Gross household income (2010 CHF, annual)	108'641	146'193	130'578	66'937
French				
	Remained renter	Became owner	Remained owner	Became renter
Observations	1'519	62	1'738	37
Age (household head)	51.5	44.4	54.7	53.7
Household size (persons)	2.2	3.0	2.8	2.2
In a couple (%)	50.4	75.8	83.8	48.6
Married (%)	40.5	66.1	79.9	45.9
Working (%)	65.7	77.4	66.5	70.3
Gender (% male)	35.9	37.1	41.0	32.4
Domestic citizenship (%)	87.9	88.7	94.0	94.6
Gross household income (2010 CHF, annual)	96'187	121'153	122'069	105'527
Italian				
	Remained renter	Became owner	Remained owner	Became renter
Observations	332	16	361	8
Age (household head)	56.2	45.7	57.6	55.4
Household size (persons)	2.1	2.8	2.6	2.0
In a couple (%)	50.0	75.0	80.1	37.5
Married (%)	42.5	56.3	81.7	37.5
Working (%)	45.2	62.5	53.7	62.5
Gender (% male)	40.1	37.5	42.9	37.5
Domestic citizenship (%)	53.3	75.0	78.4	75.0
Gross household income (2010 CHF, annual)	70'599	111'806	95'714	88'085

Source: SHP. *Notes:* Averages for households interviewed between 2002-2016. Medians for income measures. Changes in tenure refer to changes since the last survey. Real incomes are deflated by the national CPI. The sample consists of 19,474 (out of 45,816) households that report their main language. The descriptive statistics for German speakers coincide with those in the previous table by construction because the location of residence in Switzerland implies assignments only to the other three language groups.

Table 2.37: *Regional split for Italy based on the normalized measure of financial development in Guiso et al. (2004), table 2.*

Region	Measure of financial development	Classification
Marche	0.59	Developed
Liguria	0.59	Developed
Emilia - Romagna	0.52	Developed
Veneto	0.52	Developed
Piedmont	0.47	Developed
Trentino - Alto Adige	0.46	Developed
Lombardy	0.44	Developed
Friuli - Venezia Giulia	0.41	Developed
Umbria	0.40	Developed
Sardinia	0.37	Less developed
Tuscany	0.36	Less developed
Abruzzo	0.36	Less developed
Basilicata	0.35	Less developed
Molise	0.25	Less developed
Sicily	0.21	Less developed
Apulia	0.17	Less developed
Lazio	0.07	Less developed
Campania	0.03	Less developed
Calabria	0.00	Less developed

Table 2.38: *Summary statistics for housing-tenure groups across Italian regions*

	Developed				Less developed			
	Renters		Owners		Renters		Owners	
	Remained renter	Became owner	Remained owner	Became renter	Remained renter	Became owner	Remained owner	Became renter
Observations	2,756	219	11,746	155	2,417	253	10,084	157
Age (household head)	55.1	54.1	60.3	57.1	56.6	54.4	60.4	56.4
Household size (persons)	2.3	2.2	2.5	1.9	2.8	2.7	2.7	2.0
In a couple (%)	52.2	51.1	70.9	36.8	63.4	64.4	70.7	41.4
Married (%)	51.9	48.9	70.2	36.1	62.0	63.2	70.3	39.5
Working (%)	53.3	58.4	41.3	49.0	35.9	47.0	36.7	47.1
Gender (% male)	53.9	55.3	63.0	53.5	51.2	60.1	58.1	50.3
Birth region domestic (%)	81.4	91.3	97.5	89.7	93.9	96.8	98.9	98.1
Net household income (2010 EUR, annual)	19,098	30,818	36,191	16,954	14,920	22,235	27,374	15,600
Net worth (2010 EUR)	6,616	178,652	256,141	8,230	2,941	144,584	202,317	3,197

Sources: SHIW (Italy). *Notes:* Averages for households interviewed between 2002-2016. Medians for income and net worth. Changes in tenure refer to changes since the last survey. Real incomes and net worths are deflated by the national CPI.

Table 2.39: *Housing and mortgages across Italian regions*

	Developed	Less developed
Homeownership rate (%)	78.3 (0.5)	80.7 (0.5)
Incidence of mortgagors (as % owners)	17.5 (0.5)	14.2 (0.5)
Incidence of mortgagors (as % new owners)	23.1 (6.8)	16.1 (5.0)
Flexible rate mortgagors (as % of mortagors)	38.6 (1.4)	22.0 (1.3)

Sources: SHIW. *Notes:* Standard errors in parentheses. The statistics are based on the representative sample years 2014/2016 to be comparable with the aggregate statistics in Table 2.1 in the main text. New owners are renters who became owners between the last and the current survey wave.

2.10.6 Annualization of the biannual transition probabilities for Italy

For completeness, we discuss how we obtain the annual transition probabilities based on information on the biannual transition probabilities. We denote the annual transition matrix by $\mathbf{T_1}$ and the biannual transition matrix by $\mathbf{T_2}$ with

$$\mathbf{T_1} = \begin{array}{c} \\ \text{Renter} \\ \text{Owner} \end{array} \begin{array}{c} \begin{array}{cc} \text{Renter} & \text{Owner} \end{array} \\ \begin{pmatrix} (1-p) & p \\ q & (1-q) \end{pmatrix} \end{array}, \quad \mathbf{T_2} = \begin{array}{c} \\ \text{Renter} \\ \text{Owner} \end{array} \begin{array}{c} \begin{array}{cc} \text{Renter} & \text{Owner} \end{array} \\ \begin{pmatrix} (1-P) & P \\ Q & (1-Q) \end{pmatrix} \end{array}.$$

Observing that

$$\mathbf{T_2} = \mathbf{T_1}^2 = \begin{pmatrix} (1-p)(1-p) + pq & (1-p)p + p(1-q) \\ q(1-p) + (1-q)q & qp + (1-q)(1-q) \end{pmatrix} \quad ,$$

we obtain a system of two equations which maps the annual transition probabilities p and q into the corresponding biannual transition probabilities P and Q:

$$P = (1-p)p + p(1-q), \tag{2.2}$$

$$Q = (1-q)q + q(1-p). \tag{2.3}$$

The probabilities $1-P$, $1-Q$, $1-p$ and $1-q$ result from the property that probabilities in each row of the transition matrices have to sum to one.

Solving equations (2.2) and (2.3) for p and q, respectively, we obtain

$$p = \frac{2-q}{2} - \sqrt{\frac{(2-q)^2}{4} - P}, \tag{2.4}$$

$$q = \frac{2-p}{2} - \sqrt{\frac{(2-p)^2}{4} - Q}, \tag{2.5}$$

where we only consider the sign of the root for which $0 \leq p \leq 1$ and $0 \leq q \leq 1$.

For annual transitions across housing tenure states that are small, $(2-x)/2 \approx 1$ for $x = p, q$, and we obtain the closed form solutions

$$p \approx 1 - \sqrt{1-P}, \tag{2.6}$$

$$q \approx 1 - \sqrt{1-Q}. \tag{2.7}$$

In Figure 2.4 we annualize the biannual flows using equations (2.6) and (2.7), respectively. A first-order Taylor approximation of (2.6) and (2.7) implies, as usual, that

$$p \approx \frac{P}{2} \text{ and } q \approx \frac{Q}{2} \quad , \tag{2.8}$$

if biannual transition probabilities are small. We find that the approximation implies negligible approximation error in our application. We thus use (2.8) to annualize the cumulative effects of 25 bp rate cuts for the housing-tenure regressions in Italy.

3 On the Labour Market Effects of Monetary Policy across the Income Distribution

Abstract:
I estimate the effect of monetary policy on income, hours and employment across the income distribution in Switzerland, a country with a high employment rate and a high level of working hours above the European average. I show that, in line with findings from other countries, policy rate cuts lead to lower household income inequality. The reduction of income inequality is caused by an increase of labour income and labour supply in the bottom quartile of the income distribution, both at the intensive and extensive margin. The gains are concentrated at individuals that work in more elementary occupations and in the Italian- and French-speaking regions of Switzerland.

JEL Codes: E4, E52, D31
Keywords: monetary policy, income inequality, labour supply

3.1 Introduction

The debate on economic inequality has taken centre stage not only in academic research but also in central banks (see for instance Lagarde, 2020), associated with recent increases in income inequality in many OECD countries. In the US, top income shares have increased since the 1980s (Piketty et al., 2018). Also in Switzerland, top income shares have increased from the 1990s until the Great Recession as documented by Foellmi and Martínez (2017a) and Foellmi and Martínez (2017b). In this chapter, I provide evidence that aggregate changes in inequality mask underlying changes in individual and household incomes and how they are distributed across regions, changes in labour supply across professions and the distribution of work within households.

The analysis contributes to the literature that investigates drivers of inequality. One potential source of changes in inequality in the short to medium term is monetary policy. By determining financing conditions in the economy, central banks affect differently households depending on their characteristics. For example, Kaplan et al. (2018) show that the consumption response to interest rate cuts depends on the incidence of borrowing constraints across agents.

The growing evidence of heterogeneous effects of monetary policy, further discussed in section 3.2, has increased the interest of the public and central banks in the distributional consequences of monetary policy. Central banks may not be interested in inequality per se, because it is beyond their mandate. Inequality is of interest for monetary-policy makers, however, to the extent that it shapes the transmission and the effectiveness of monetary policy decisions on the macroeconomy.

Furthermore, there are concerns that rising income inequality has contributed to the reduction of the natural real interest rate, a phenomenon also associated with secular stagnation, because individuals with high incomes tend to have higher saving rates than low income individuals (Auclert and Rognlie, 2017). An increase of income inequality is thus also of interest for monetary-policy makers because it may change the probability of reaching the effective lower bound on interest rates and thus constrain the policy space.

The evidence on the labour market effects of monetary policy in this chapter complements findings of recent structural models with heterogeneous agents. The structural evidence emphasizes the labour market effects of monetary policy as an important indirect channel of the transmission of monetary policy to consumption (Kaplan et al., 2018). Empirically, Holm et al. (2021) show that after two years, the indirect labour income effect dominates the direct effect of intertemporal substitution on consumption. This chapter contributes to this literature by documenting large labour market effects of monetary policy across the income distribution. The sizable effects illustrate that there may be scope for coordinating monetary and fiscal policy.

In this chapter, I measure the implications of monetary policy for the distribution of income, hours and employment in Switzerland. Switzerland is an interesting country to analyze for at least four reasons. First, although Switzerland possesses an independent monetary authority, the Swiss National Bank (SNB), the consequences of monetary policy on the income distribution in Switzerland have not been studied to date. I provide evidence that, similar to studies conducted on other countries, expansionary monetary policy leads to a reduction of household income inequality. The inequality reduction is large and significant in the bottom half of the household income distribution. Specifically, I find that policy rate cuts increase household incomes in the bottom income quartile with most of the effect taking place two years after the shock , while household incomes in the top three quartiles are barely affected. At the individual level, a policy rate cut leads to an increase of labour income of the employed in the bottom half of the household income distribution.

Second, the unique nature of the Swiss labour market with its high employment rate, low unemployment rate and relatively high working hours has been documented in the recent literature by Bick et al. (2018), Bick et al. (2019) and Rogerson and Shimer (2011). In my analysis, I show that expansionary monetary policy not only affects the extensive margin of labour supply, by increasing the employment rate of those living in low income households. It also has an impact on the intensive margin, by increasing the number of hours worked. The induced increase in hours of employed individuals is driven by the main earners of the household. Further, the increase is concentrated at individuals who report to work in more elementary professions. These findings suggest that even in a country with high employment, monetary policy affects incomes and employment in those households, in which employment is comparably low.

Third, the use of the Swiss Household Panel (SHP) allows to group households or individuals into household income groups prior to the monetary policy shocks. Additionally, the panel structure offers the opportunity to estimate the effect of monetary policy on labour market transitions. I find that the positive employment effect of policy rate cuts observed for low income households is brought about by an increase in the transition from unemployment to employment as well as a decrease of the transition in the opposite direction.

Fourth, as a country with four official languages, Switzerland exhibits high regional diversity. This feature is reflected in the regional effects of monetary policy. I find that expansionary monetary policy benefits most households in the Italian- and French-speaking regions. These regions are also overrepresented as regions of residence for households in the bottom income quartile.

The theoretical literature suggests that expansionary monetary policy can increase hours and wages by boosting aggregate demand and thereby reduce income inequality, as discussed in section 3.2. Even though the empirical literature provides a lot of evidence for the reduction in income inequality through a policy rate cut, there is limited evidence about the transmission channels that drive this effect. My analysis partly fills this gap by investigating not only the inequality effects of monetary policy, but also the effects on incomes and hours at the individual level across the household income distribution.

I identify monetary policy shocks using financial market expectations on interest rates as in Altavilla et al. (2019) or Corsetti et al. (2021) for the euro area and Nakamura and Steinsson (2018a) or Wong (2021) for the US. The identifying assumption is that there is no endogenous component driving monetary policy decisions that is not adequately included into future prices at the moment of the announcement. Other studies, such as Coibion et al. (2017), use the narrative approach to identify monetary policy shocks, specifying a model that describes assumptions about how economic conditions cause policy rate changes. Using a high-frequency approach has the advantage that I do not need to make assumptions about the nature of this model.

The chapter is structured as follows: Section 3.2 summarizes the theoretical and empirical evidence on the distributional consequences of monetary policy with a focus on income inequality. I discuss the high-frequency identification of monetary policy shocks in section 3.3. Section 3.4 provides an overview of the panel data set and household income inequality in Switzerland. The main results are presented in section 3.5. Section 3.6 concludes.

3.2 Literature

A growing literature deals with the heterogenous effects of monetary policy across households with different characteristics. Beraja et al. (2019) document differences in the regional transmission of monetary policy in the US during the Great Recession, focusing on loan-to-value ratios. Kaplan et al. (2018) show that the consumption responses to interest rate changes vary across households with different marginal propensities to consume. Empirically, Holm et al. (2021) find with Norwegian data that the consumption effects of monetary policy shocks are largest for households with strong interest rate exposure. Adam and Zhu (2016) show that the gains and losses from unexpected price level changes are unequally distributed within and between euro area countries. Unexpected price level changes can in turn be the consequence of unexpected policy rate changes.

These analyses relate to this chapter in two ways. First, I contribute to the literature by documenting the income and hours effects of monetary policy across households and individuals with different characteristics, such as income and profession. Second, these studies underline the policy relevance of the research question. The distributional consequences of monetary policy are not only interesting per se, they also matter for the subsequent effectiveness of monetary policy to affect for instance consumption.

This chapter also complements research on the heterogeneity effects of monetary policy using structural models. Using a New Keynesian model with matching frictions and household heterogeneity, Gornemann et al. (2021) show that an increase of the policy rate leads to an increase of the income Gini coefficient. They thus find that expansionary monetary policy decreases income inequality. A key feature of the model is a job-finding rate that depends on the state of the business cycle so that an increase of the policy rate leads to a decrease of the wage rate. Luetticke (2021) finds that in a heterogeneous agent New Keynesian model, an increase of the policy rate leads to an increase of the income Gini coefficient. Similarly, the heterogeneous agent New Keynesian model by Bayer et al. (2020) suggests that a monetary tightening should increase the income share of the top 10%.

A number of empirical studies investigate the inequality effects of monetary policy. In line with the findings in this chapter, most of them conclude that expansionary monetary policy reduces income inequality. Using the Consumer Expenditure Survey (CEX), Coibion et al. (2017) find that contractionary monetary policy shocks increase income inequality in the US. Specifically, they find that a 10 basis points cut leads to a reduction of the income Gini coefficient by 0.1 percentage points.[1]

Using data from the Family Expenditure Survey (FES), Mumtaz and Theophilopoulou (2017) find that in the UK, contractionary monetary policy leads to falling incomes at the bottom of the distribution, whereas they do not fall by much at the top. Using a structural vector autoregression (VAR) they find that a 10 basis points cut leads to a reduction of the income Gini coefficient by between 0.09 to 0.3 percentage points.[2]

[1] Coibion et al. (2017) state that a positive 1.5 percentage point monetary policy shock leads to an increase of the income Gini coefficient of 1.5 percentage points.

[2] Mumtaz and Theophilopoulou (2017) state that a positive 1 percentage point monetary policy shock leads to an increase of the income Gini coefficient of 0.9 to 3 percentage points.

Furceri et al. (2018) and Samarina and Nguyen (2019) both provide a cross-country analysis on the inequality effects of monetary policy using the Gini coefficients from the Standardized World Income Inequality Database. Furceri et al. (2018) study 32 advanced and emerging market countries and conclude that contractionary monetary policy increases inequality by more than expansionary monetary policy lowers it. Samarina and Nguyen (2019) find that expansionary monetary policy reduces income inequality. They estimate that a 10 basis points cut leads to a reduction of the income Gini coefficient by 0.1 percentage points. They also find that expansionary monetary policy increases employment in the euro area. I find that expansionary monetary policy leads to an increase in hours and employment also in Switzerland.

Amberg et al. (2021) estimate the income effects of expansionary monetary policy across the income distribution in Sweden. They show that labour income increases at the bottom of the distribution while capital income increases at the top leading to a U-shaped effect on total income over the income distribution. In line with these findings, the results in this chapter also suggest that expansionary monetary policy boosts incomes at the bottom and reduces income inequality in the bottom half of the income distribution.

Lenza and Slacalek (2018) investigate the effects of asset purchase programs on incomes and wealth across the distribution in the euro area. They find that asset purchases have lead to income growth that is concentrated at the bottom quintile of the income distribution. The differential effect is stronger in the euro area's periphery, where unemployment rates are higher across all income quintiles.

Andersen et al. (2021) use administrative household data from Denmark to study the income effects of expansionary monetary policy. In contrast to most of the literature, they find that expansionary monetary policy leads to a strong increase of disposable income at the top of the income distribution, while disposable incomes at the bottom of the distribution barely increase.

This chapter adds to the literature on the inequality effects of monetary policy in at least three ways. First, the data from the SHP allows to investigate the channels through which monetary policy affects income inequality. I find that expansionary monetary policy promotes employment at the extensive and hours at the intensive margin for individuals in the bottom quartile of the household income distribution. Furthermore, I show that the effect of monetary policy on incomes and hours varies across professions and regions. I find that individuals in more elementary occupations as well as households in Italian- and French-speaking regions benefit most from expansionary monetary policy. This finding adds another dimension to the finding of reduced inequality in the aggregate economy.

Second, most of the existing literature uses a narrative approach or VARs to identify monetary policy shocks. A potential drawback of these approaches is that one needs to make assumptions about which variables parametrically determine the central bank's decision. In this chapter, I use a high-frequency approach to identify the shocks, which allows to be agnostic about the endogenous drivers of monetary policy decisions.

Third, there is no study to this date that estimates the effects of monetary policy on income inequality in Switzerland.

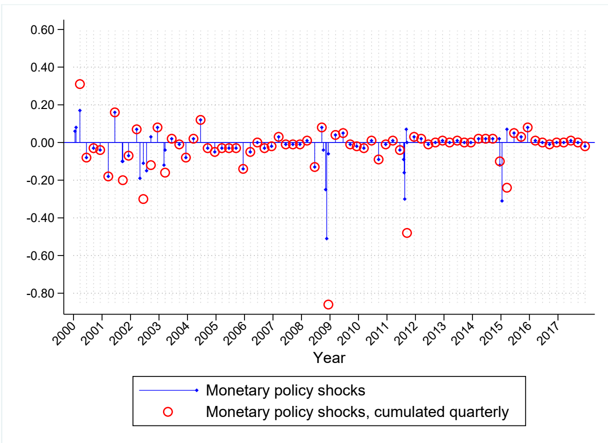
3.3 Monetary policy shocks

The empirical investigation of monetary policy transmission calls for the identification of monetary policy shocks so that the estimated coefficients can be interpreted as causal effects. Policy rate changes do not fulfill this criterion because they also include an endogenous element, as central banks react to prior or expected economic conditions.

One way to address this issue was pioneered by Romer and Romer (2004). They regress policy rate changes against a number of economic variables and economic forecasts from the FED. Under the assumption that all relevant decision variables are included in the regression, they use the residuals as exogenous monetary policy shocks.

An alternative way to identify exogenous changes in the policy rate is the high-frequency identification. This method is used in a number of recent studies, including Altavilla et al. (2019) for the euro area and Nakamura and Steinsson (2018a) or Wong (2021) for the US.

In this chapter, I use the series of monetary policy shocks as constructed in Koeniger et al. (2021a). I identify monetary policy shocks with high-frequency data, namely the daily change of the implied rate of futures on the 3-month Swiss-Franc Libor on dates of monetary policy announcements. The identifying assumption is that there is no variable that can systematically predict these shocks and is also correlated with the dependent variables investigated in this chapter. As a robustness check, I use a six hour time window around the announcement, as in Corsetti et al. (2021).



Sources: Futures contracts' prices from TickDataMarket; series constructed as in Koeniger et al. (2021a). *Notes:* The blue droplines show the monetary policy shocks on announcement dates constructed using data on futures contracts for the 3-month Swiss-Franc Libor. The red circles show the cumulated shocks by quarter at the last announcement date of the quarter.

Figure 3.1: *Monetary policy shocks*

The identifying assumption can be justified by the nature of the shocks' construction. Market participants build expectations about the future short term interest rate based on all publicly available information, including expectations about the future state of the economy. So, changes in the implied rate on announcement dates reflect an exogenous shock that must be independent of all available information at the time before the announcement. Table 3.4 in Appendix 3.7.1 tests whether lagged growth rates of GDP or consumption expenditures in Switzerland can predict the quarterly cumulated monetary policy shocks. The reassuring finding is that there is no significant correlation between the lags one to four of GDP and consumption growth and monetary policy shocks.

Table 3.1: *Monetary policy shock statistics*

	Summary statistics	
Mean	-0.0339	
Standard deviation	0.1409	
Min	-0.8600	
Max	0.3100	
	Coefficient	Significance level
Autocorrelation with shock Q(-1)	-0.1849	0.1226
Autocorrelation with shock Q(-2)	-0.0310	0.7990
Autocorrelation with shock Q(-3)	0.0460	0.7076
Autocorrelation with shock Q(-4)	-0.1164	0.3444

Sources: Futures contracts' prices from TickDataMarket; series constructed as in Koeniger et al. (2021a). *Notes:* The upper panel shows summary statistics for 72 monetary policy shocks cumulated at the quarter between 2000Q1 and 2017Q4. The lower panel shows the correlation of these cumulated monetary policy shocks with its lags and the respective significance level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.1 shows the monetary policy shocks obtained from changes of the implied rate of futures on monetary policy announcements dates. Table 3.1 shows descriptive statistics for the monetary policy shocks cumulated at the quarter. The shocks fluctuate around zero, a finding that suggests that market participants do not form systematically biased expectations about interest rates. Cumulating the shock at the quarterly frequency does not change the overall picture of shocks in Figure 3.1, also because monetary policy announcements in Switzerland take place once a quarter in usual times.

One concern with the identification strategy outlined above is that the central bank may possess private information about the (future) state of the economy and reveal that information at monetary policy announcements. Consequently, market participants would not only react to a potential change in the policy rate but also to update their expectations about the economy. As discussed by Koeniger et al. (2021a) in reference to Wong (2021), the existence of private information guiding the central bank's decision should lead to some autocorrelation of monetary policy shocks. The lower panel of Table 3.1 provides additional evidence to check the concern that the monetary policy shocks could be autocorrelated. None of the correlation coefficients between shocks and their lags up to four quarters is significant at the 10% level.

Jarociński and Karadi (2020) find that high-frequency monetary policy shocks can be decomposed into an exogenous element (interest rate shock) and an element that captures the revelation of central bank information about the state of the economy (news shock). To capture this distinction made in the literature, I perform a robustness check of the main specification. I define interest rate shocks as monetary policy shocks that have the opposite sign as the return of the Swiss stock market on the same date. News shocks are defined as shocks that have the same sign as the stock market return.

Ceteris paribus, an interest rate cut should lead to higher asset prices, because future cash flows are discounted at a lower rate. News shocks therefore suggest that the monetary policy announcement revealed information to market participants about expected future cash flows. Figure 3.12 in Appendix 3.7.1 shows the decomposition of the cumulated monetary policy shocks into these two categories.

Due to the time span of the data (2000-2017), I cumulate the past monetary policy shocks over separate years for lags up to three years. Figure 3.16 in Appendix

3.7.3 provides a robustness check for the main specification using a shorter lag length of two years.

In order to provide a benchmark for the plausibility of this procedure, Table 3.5 in Appendix 3.7.1 shows the results of regressions of GDP and consumption growth rates in Switzerland against three years of lagged monetary policy shocks. The results are economically plausible and qualitatively similar to other models of the Swiss economy, such as Kugler et al. (2005). A 10 basis points cut increases GDP and consumption growth rates by 0.20 and 0.12 percentage points respectively, although the effect is not significant at the 10% level. The effect on both GDP and consumption growth is largest in the second year.

3.4 Data

The SHP is an unbalanced household panel that starts in 1999. In this chapter, I use both the personal and the household information from the data set. I exclude household (and their member) observations who report non-positive household income. Further details on data cleaning and sample selection can be found in Appendix 3.7.4.

The SHP has a number of advantages for the investigation of the labour market effects of monetary policy. The panel structure allows to estimate the effects of monetary policy conditional on household income groups *before* the shocks happened. It also allows to study how the transitions between employment states are affected by monetary policy.

The data set allows to construct household income inequality measures. In line with the literature, I look at gross household incomes, which are adjusted for household size using the equivalent scales, specifically the means over different scales in Table 1 in Fernández-Villaverde and Krueger (2007).[3] Since the SHP is a survey-based data set, I focus on inequality measures that do not require precise measurement of the distribution's tails.

[3]For household sizes greater than 5, an additional 0.3 is added for each person.

Throughout this chapter, I use the quarterly variation from the dataset inferred from the interview dates.[4] Figure 3.2 shows the evolution of several income inequality measures computed at each quarter over time. The income ratios can be read as follows: In 2003, a household at the 90th percentile of the household income distribution earned roughly 3.5 times as much as a household at the 10th percentile of the household income distribution in the same quarter. Two things are noteworthy. First, the trend of income inequality measures from the SHP is relatively flat, only trending upwards slightly for some measures. Second, inequality measures retrieved from the SHP tend to be low. As an example, the Gini coefficient is around 0.3 during the whole sample period.

Sources: SHP. *Notes:* Inequality measures are computed every quarter using yearly gross household income in equivalent scales for households with positive reported household income.

Figure 3.2: *Household inequality measures based on SHP*

[4] Due to the setup of the survey, there are almost no observations in the second quarters. Figure 3.13 in Appendix 3.7.1 shows boxplots of the monetary policy shocks by quarter. It shows that the shocks do not systematically differ across quarters. Table 3.6 in Appendix 3.7.2 provides the summary statistics of Table 3.2 over the three remaining quarters. It shows that household and individual characteristics do not systematically vary over interview date quarters.

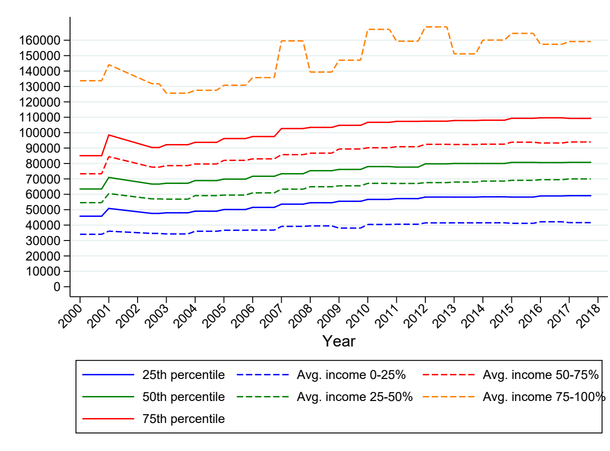
How do the inequality measures in Figure 3.2 compare with inequality trends documented in the literature? In Switzerland, there is evidence of an increase in top income shares from the beginning of the 21st century until the Great Recession as documented by Foellmi and Martínez (2017a) and Foellmi and Martínez (2017b). Foellmi and Martínez (2017a) show that more coarse inequality measures such as the income share of the top 10% only show a smaller uptick during that period. This is in line with a small increase of the coarse inequality measures retrieved from the SHP, such as the ratio of the 90th to 10th percentile, which also exhibit a slight increase until the Great Recession. The standard deviation of log income is slightly smaller than that documented for other European countries (Krueger et al., 2010, Table 4). Specifically, Krueger et al. (2010) document that in the UK the standard deviation of log income is 0.74, while it is between 0.5 and 0.6 in most of the SHP sample. Similar to other European countries, inequality at the bottom of the income distribution (ratio 50 to 10) is larger than at the top of the income distribution (ratio 90 to 50) (Krueger et al., 2010, Table 5).

As noted before, the Gini coefficient obtained from the SHP is lower than those from other data sources. This pattern is also documented by Suter and Gazareth (2016). A potential explanation is that the SHP data is based on a survey with few households sampled at the top tail of the distribution thus underestimating income inequality at the top.

For my analysis, I divide households into income quartiles, so the $0-25\%$, $25-50\%$, $50-75\%$ and $75-100\%$ of the income distribution. This division allows to have a sufficiently large sample size per bin. I follow the literature and group households by gross household income in adult equivalents to account for differences in household size.

Figure 3.3 shows the evolution of household income at the 25th, 50th and 75th household gross income percentile, as well as the average household gross income within each income quartile. Income at all percentiles of the income distribution is increasing over time. Average income in the top quartile of the income distribution is more volatile than in lower income quartiles. This is not surprising, because outliers affect averages at the top of the household income distribution.

Households at all three income percentiles had almost between 27 and 29 percent higher nominal incomes in 2017 than in 2000.



Sources: SHP. *Notes:* The figure displays the 25th, 50th and 75th percentile of the gross household income distribution in equivalent scales together with the average gross household income in equivalent scales in the four household income quartiles.

Figure 3.3: *The evolution of household income across the income distribution over time*

Table 3.2 summarizes the characteristics of households in each income quartile. The upper panel shows the means of real gross total household income in adult equivalents and the number of persons in the household in each household income quartile, averaged over all household in the respective quartile. The lower panel shows a descriptive statistics of individual characteristics in each household income quartile, averaged over all persons with age between 20 and 65 in each respective household quartile.

The upper panel of Table 3.2 shows an intuitive and interesting feature of between household inequality in Switzerland. Households at the bottom of the household income distribution are larger than households at the top of the household income distribution. Hence gross household income in equivalized units is lower, ceteris paribus. The labour supply of additional of additional household members does not compensate this mechanical effect in Switzerland.[5]

The lower panel of Table 3.2 offers insights into the differences between individual characteristics within household income groups. Individuals in the top income quartile on average are the oldest in the sample, they have the most years of education, describe themselves less likely as female, are least likely to be married and to have children and most likely have the Swiss nationality compared with the other household income groups.

As one would expect, individuals in lower household income groups earn lower labour incomes. In contrast, social transfer incomes are higher in low household income groups, in line with an employment rate that increases with household income. Incomes from other sources are highest in the top income quartile. Presumably, capital income from sources such as intergenerational transfers or rental income from real estate is also highest in this income group.

There is also heterogeneity in labour supply, both at the extensive margin (employment) and intensive margin (hours). As discussed before, the employment rate is highest in the top quartile of the income distribution with around 91% of working age adults. The average number of hours worked for employed adults also increases in household income. More employed adults in high income households work full time than in low income households.

[5] See Krueger et al. (2010) for a discussion of cross-country differences in the relationship between household earnings and individual earnings inequality.

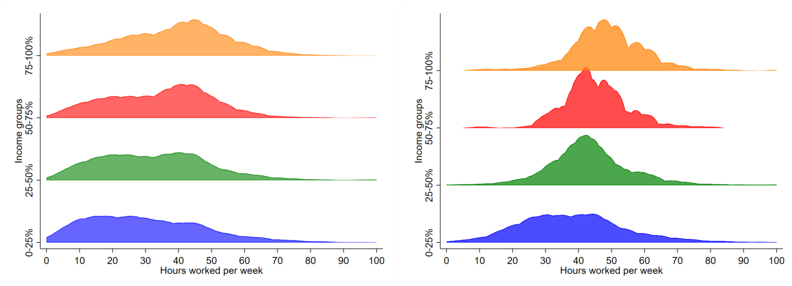
Table 3.2: *Summary statistics*

Income groups	0-25%	25-50%	50-75%	75-100%
Household characteristics				
Real gross total household income in eq. scales	40,695	67,398	91,268	155,547
Number of persons in household	2.6	2.3	2.0	1.9
Individual characteristics				
Real gross work income	32,955	54,887	70,059	102,932
Real social public transfer income	1,787	1,003	971	967
Real informal transfer income	2,331	2,025	1,904	1,937
Real income from other sources	3,320	4,381	5,578	11,834
Employment rate	72.9	84.8	89.2	91.1
Hours worked per week (>0)	30.3	32.7	34.5	39.0
Hours worked per week (>0, full time)	49.7	51.2	49.3	51.3
Full time (% of employed)	39.6	48.0	53.5	60.2
Age	43.9	44.4	44.6	46.1
Years of education	12.7	13.5	14.1	15.2
Female (%)	63.9	56.1	52.7	49.2
Married (%)	62.4	65.0	59.4	58.4
Has children (%)	74.9	71.5	62.7	52.9
Swiss nationality (%)	85.9	89.7	91.2	91.2
Observations	13,752	15,616	16,889	18,344

Sources: SHP. *Notes:* The upper panel shows means for households interviewed between 2002-2017. The lower panel shows means for individuals interviewed between 2002-2017. Real incomes deflated by the CPI. See Appendix 3.7.4 for further details on the construction of the variables and the sample.

Figure 3.4 provides more details on this observation. It shows the distribution of hours worked of all employed persons (left panel) and main earners in the households (right panel). A household's main earner is defined as a household member who earns at least half of total gross household income with her labour income. In line with the means shown in Table 3.2, reported hours worked increase with household income.

In the lowest household income group, there are many persons working part time with around 20 hours per day. Concerning main earners, a large fraction of persons in the lowest income quartile work less than 40 hours per week. There is even heterogeneity at the top of the income distribution. Most of main earners in the top household income quartile report a working time of more than 40 hours per week, while the distributions in the second and third household income quartiles are more centered around a working time of 40 hours per week.



Sources: SHP. *Notes:* The left panel shows the distribution of positive hours worked per week of individuals in the household income quartiles. The right panel shows the distribution of positive hours worked per week of individuals classified as main earners in the household income quartiles. A household's main earner is defined as a household member with gross labour income greater or equal to gross household income.

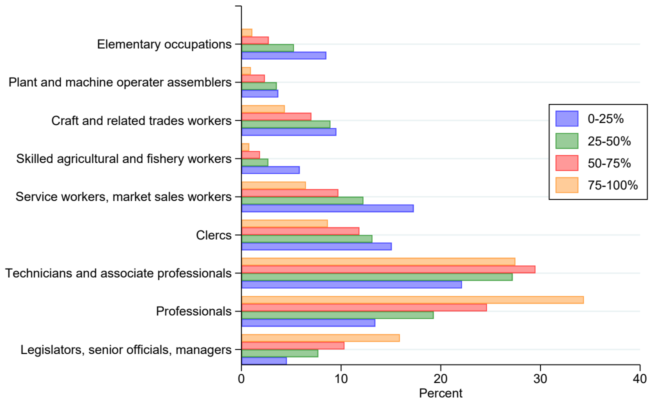
Figure 3.4: *Hours worked in income groups for all working persons (left) and main earners (right)*

How do the stylized facts about employment and hours worked compare with the evidence on labour force participation and working hours in Switzerland and worldwide? Bick et al. (2019) document that Switzerland has the highest average hours worked per year in Western Europe. This finding can be decomposed into hours worked per week above the European average, in line with Figure 3.4, and the highest European employment rate, that is also documented in Table 3.2. Specifically, Bick et al. (2019) document that Swiss adults work on average around 1,300 hours per year, an average higher than in the US. As a comparison, in a country with low average working hours in Europe, Italian adults work fewer than 900 hours per year.

Bick et al. (2018) find that across the world, the within country correlation between wages and hours worked is mostly negative. For Switzerland, they find a correlation close to zero. Specifics of the Swiss labour market are also documented by Rogerson and Shimer (2011). They show that most of the cyclical variation of total hours worked is driven by movements from employment to non-participation in the labour force and vice versa at constant hours per worker, while it is driven by flows into and out of unemployment for other countries.[6] In section 3.5, I therefore consider both the effect of monetary policy shocks on hours of the employed and on the employment rates as well as transitions across the three employment states.

Figure 3.5 shows the distribution of professions across individuals in household income groups. In the bottom three income quartiles, most individuals are technicians and associate professionals, while most individuals in the top income quartile are professionals. A larger fraction of individuals in the bottom household income quartile works in professions such as elementary occupations, skilled agricultural and fishery workers and service workers or market sales workers. Figure 3.14 in Appendix 3.7.2 shows household regions of residence across the income distribution. In line with the regional economic heterogeneity in Switzerland, lower income households are overrepresented in the French- and Italian-speaking parts of Switzerland. Individuals in the top income quartile more likely live in Zurich or Central Switzerland than individuals from other household income groups.

[6] In the sample, the unemployment rate is at 2% and increases to 5% in the lowest household income quartile.



Sources: SHP. *Notes:* The figure shows the distribution of professions, the reported ISCO classifications in the main current job (1-digit-position) of individuals in the household income quartiles.

Figure 3.5: *Professions across household income groups*

3.5 Results

3.5.1 Effect on inequality measures

First, let us turn to the effect of monetary policy on income inequality. As outlined in section 3.2, most studies conclude that expansionary monetary policy leads to a fall in income inequality. Usually, the effect of monetary policy shocks on these inequality measures wears off after two to three years. Since the time span of the data set is comparably short, I cumulate the shocks to an annual frequency to increase the degrees of freedom in the estimation. Additionally, this allows me to control for seasonality and trends. Specifically, I estimate the following equation:

$$\mathrm{I}_{qt} = \alpha + \beta' \mathbf{MP}_{qt} + F_q + F_t + \varepsilon_{qt}. \tag{3.1}$$

I_{qt} is the inequality measure in quarter q of year t. The vector $\mathbf{MP}_{qt}$ denotes the monetary policy shocks in the last three years, cumulated to a quarterly frequency separately for each of the years. I control for common effects by quarter F_q and year F_t, and thus control for trends and seasonal effects.

Table 3.3: *The effect of monetary policy shocks on income inequality measures*

	Ratio 90 to 10	Ratio 90 to 50	Ratio 50 to 10	Ratio 75 to 25	Gini	SD of log income
Monetary policy shock, sum Q(-1,-4)	0.749	-0.081	0.457	0.118	0.049	0.134
Monetary policy shock, sum Q(-5,-8)	0.848	-0.090	0.573**	0.067	-0.005	0.084
Monetary policy shock, sum Q(-9,-12)	1.344*	0.111	0.586**	0.258*	-0.016	0.122
Quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45	45	45	45	45	45
Cum. effect of 10 bps cut	-0.294	0.006	-0.161***	-0.044	-0.003	-0.034

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the respective household income inequality measure. Robust standard errors. The cumulative effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1.

Table 3.3 shows the estimation results for equation (1). Although the sample size is small, some of the coefficients are estimated precisely enough to be different from zero at standard significance levels. Overall, the results confirm the evidence from other countries that expansionary monetary policy reduces income inequality. As an example, a 10 basis points monetary policy cut reduces the ratio of the 90th to the 10th income percentile by around 0.3, although the cumulative effect is not estimated precisely enough to be significant at the 10%-level. The results indicate that this effect is driven by a significant reduction of the 50th to the 10th income percentile. A 10 basis points cut reduces this inequality measure by 0.16. This effect is precisely estimated and significant at the 1%-level. So expansionary monetary policy leads to a reduction in income inequality at the bottom half of the income distribution. The finding is similar to evidence provided by Mumtaz and Theophilopoulou (2017) for the UK.

The effect of monetary policy on the Gini coefficient is small and not significantly different from zero. The confidence interval of the cumulative effect includes most of the estimates discussed in section 3.2. Also, the effect on the standard deviation of log income is not significant overall, although the point estimates for all three years and the cumulative effect indicate that expansionary monetary policy reduces inequality.[7]

The results presented in Table 3.3 indicate that in Switzerland, expansionary monetary policy decreases household income inequality. Next, I investigate in further detail how monetary policy shocks affect household incomes in different parts of the household income distribution. Although the effect on income is of interest in itself, it could be caused by very different reasons. Expansionary monetary policy may lead to higher wages, more hours of the employed or a higher employment rate at the bottom of the household income distribution. Similarly, monetary policy may disproportionately affect different professions or regions in the economy, in which households from different parts of the income distribution may be differently represented.

3.5.2 Effect on household income across the income distribution

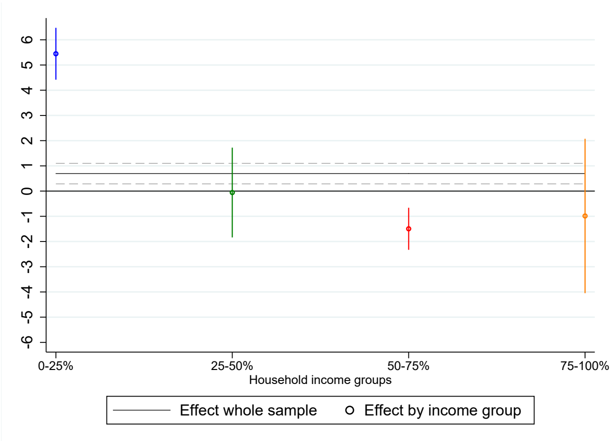
To investigate the transmission mechanism of monetary policy to the labour market in further detail, I further exploit the household data that is provided in the SHP. I assign households to household income quartiles, as outlined in section 3.4, three years before the time of observation. This approach allows to prevent that the household classification depends on the monetary policy shocks. At the same time, the classification is close enough to be informative about where households rank at the time of the shock.

First, consider for each household h the effect of monetary policy shocks on gross equivalized household income. For each household income quartile $1-4$ in year $t-3$, I separately estimate the following equation:

$$\mathrm{Y}_{hqt} = \alpha + \beta' \mathbf{MP}_{qt} + \delta' \mathbf{x}_{hqt} + F_q + F_t + \varepsilon_{hqt}. \tag{3.2}$$

[7] When controlling for the quarters, in which there are fewer than 200 observations, the cumulative effect of a 10 basis points cut is equal to -0.01 and becomes significant at the 5% level.

Y_{hqt} denotes the outcome variable, i.e., the log of gross equivalized household income. The vector $\mathbf{MP}_{qt}$ denotes the monetary policy shocks in the last three years, cumulated over quarters separately for each of the years. The vector $\mathbf{x}_{hqt}$ contains a set of control variables, which vary at the individual level. In the baseline specification, these are only age and age squared of the household's reference person to avoid introducing endogenous variables in the regression specification. As before, common effects by quarter F_q and year F_t control for seasonality and the time trend.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross equivalized household income. $\mathbf{x}_{hqt}$ includes age and age squared of household's reference person. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the household level. In total, the regressions include 13,832 observations.

Figure 3.6: *Effect of 10 bps cut across income groups on gross household income in %*

To illustrate the effects of monetary policy shocks on household incomes across the household income distribution, Figure 3.6 shows the cumulative effect of a 10 basis points cut on household income in percent for each household income group as well as for the whole sample, together with the respective 95% confidence intervals. The effect for the whole sample is positive and significantly different from zero. But income gains from expansionary monetary policy are unequally distributed across household income groups. The positive overall effect is driven by a large increase in household incomes at the bottom of the household income distribution.

The results presented in Figure 3.6 reaffirm the finding presented previously: Expansionary monetary policy reduces income inequality mainly by increasing incomes at the bottom of the income distribution. Further, it leads to an increase of incomes at the bottom of the distribution.

Figure 3.15 in Appendix 3.7.3 shows the estimated effects of a 10 basis points cut on household incomes from equation (2) using separately news and interest rate shocks. For all household income quartiles, the cumulative effects do not differ significantly. Apparently, the nature of the monetary policy shock does not have a strong influence on its effects on household income inequality.[8]

The panel data set also allows to investigate if monetary policy affects household incomes differently in different parts of Switzerland. Figure 3.19 in Appendix 3.7.3 shows the effects of expansionary monetary policy across Swiss regions. The income effect of expansionary monetary policy is largest in the regions of Ticino, Lake Geneva and Middleland. The results show that the income inequality effects of monetary policy have a regional dimension. Low income households more likely live in the regions of Ticino and Lake Geneva than other households.[9]

[8]Figure 3.16 plots the effects from equation (2) with only two years of monetary policy shocks included. The effects are only slightly smaller. Figure 3.17 reports the results of quantile regressions for incomes at the mid-percentile of the household income quartiles with similar results. Figure 3.18 replicates Figure 3.6 using shocks with a time window of six hours around the announcement. The effects are qualitatively similar but imply larger coefficient estimates than the baseline. This finding can be expected, because the shorter time window results in smaller shocks.

[9]The heterogeneity across regions is strongly reduced if I run separate regressions not reported for brevity for each household income group and region. This finding suggests that the heterogenous effect across regions results from regional income heterogeneity.

3.5.3 Effect on individual income, hours and employment across the income distribution

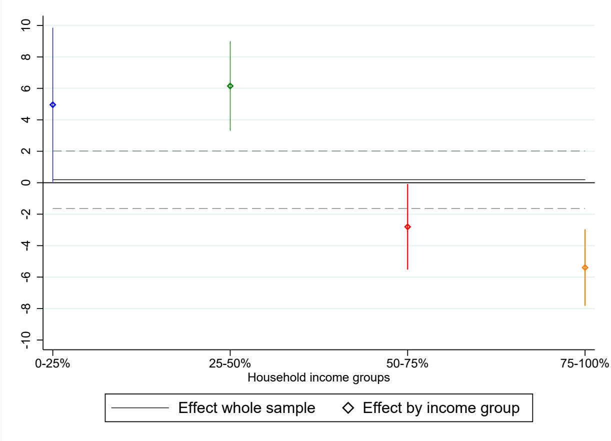
As a next step, I further dissect the household income effects of monetary policy. I use the individual information provided in the SHP to estimate the effects of expansionary monetary policy on individual incomes. Then I turn to the effect on labour supply at the intensive (hours) and extensive (employment) margin. Specifically, for each household income quartile $1-4$ in year $t-3$, I estimate:

$$\mathrm{Y}_{iqt} = \alpha + \beta'\mathbf{MP}_{qt} + \delta'\mathbf{x}_{iqt} + F_q + F_t + \varepsilon_{iqt}. \tag{3.3}$$

Y_{iqt} is the individual outcome variable at the year-quarter. The vector $\mathbf{x}_{iqt}$ contains a set of control variables, which vary at the individual level. In the baseline specification, these are age and age squared. Again, I control for common effects by quarter F_q and year F_t.

Figure 3.7 shows the cumulative effect of a 10 basis points cut on individual work income in percent for each household income group as well as for the whole sample, together with the respective 95% confidence intervals. I find that the effect on individual income is heterogeneous across quartiles of the household income distribution. The effect on individual income is positive at the bottom of the distribution and negative at the top, implying that the overall effect on individual incomes cannot be distinguished from zero at standard significance levels.

The effects shown in Figure 3.7 are robust to including individual controls, i.e. years of education, gender, marriage, children and Swiss nationality. The resulting figure is shown in left panel of Figure 3.21 in Appendix 3.7.3. Figure 3.22 in Appendix 3.7.3 also hints at heterogeneity across the age distribution. The point estimates for the income effects are largest for the bottom and top age quartiles, but do not differ significantly across age groups.

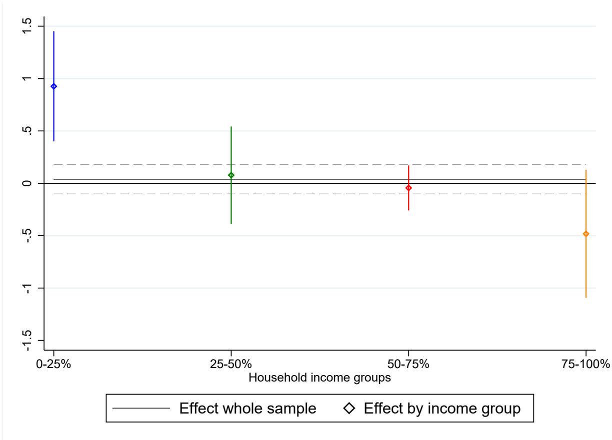


Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross work income of individuals in work. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level. In total, the regressions include 32,704 observations.

Figure 3.7: *Effect of 10 bps cut across household income groups on gross labour income of employed in %*

Overall, a substantial fraction of the income effect at the household level is driven by an increase of labour income of employed individuals in lower income groups. Figure 3.20 in Appendix 3.7.3 shows that there is no clear-cut effect on individual incomes other than labour income. Also note that in all household income quartiles labour income constitutes the largest share of gross household incomes.

After establishing that labour incomes of employed persons in the bottom half of the household income distribution increase, consider the effect on the hours of employed workers. The analysis by Gornemann et al. (2021) indicates that expansionary monetary policy leads to an increase of the wage through an increase of labour demand and thereby to higher employment. The effect on hours worked discussed below provides support for these model predictions.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is hours worked per week of individuals in work. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level. In total, the regressions include 31,195 observations.

Figure 3.8: *Effect of 10 bps cut across income groups on hours worked per week of employed*

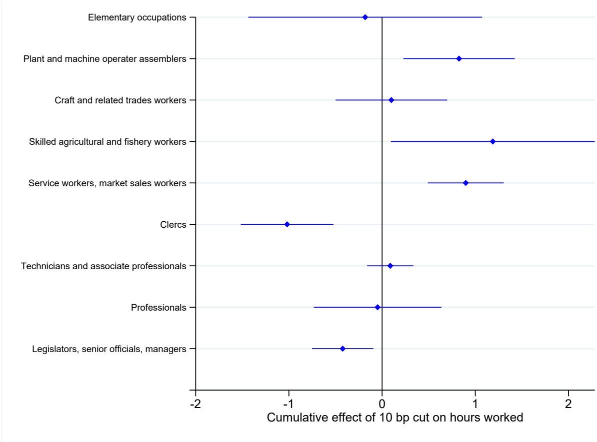
The effect of expansionary monetary policy on hours worked of employed individuals is shown in Figure 3.8. In the top three household income quartiles, there is no significant effect of a policy rate cut on hours worked. There is a positive effect for individuals in the bottom household income quartile. Specifically, a 10 basis points cut is associated with an increase of weekly working hours of employed individuals of 0.9 hours. So the positive income effect of expansionary monetary policy for low-income households is at least partly driven by an increase of labour supply at the intensive margin. The finding provides a more nuanced perspective on the findings documented by Rogerson and Shimer (2011). At the bottom of the income distribution, where average hours are lowest, monetary policy increases labour supply at the intensive margin.

The effects shown in Figure 3.8 are robust to including individual controls, i.e. years of education, gender, marriage, children and Swiss nationality. The resulting figure is shown in right panel of Figure 3.21 in Appendix 3.7.3.

The effect on the share of full-time employed as a fraction of all employed is shown in Figure 3.24 in Appendix 3.7.3. It confirms the finding that expansionary monetary policy is associated with an increase of the full-time employee share particularly in the bottom household income quartile.

Who increases hours worked after a policy rate cut? Figure 3.23 in Appendix 3.7.3 decomposes the hours effect from Figure 3.8 into the effect on the household main earner and other household members. I find that the positive effect on hours for the bottom income quartile results from an hours increase for main earners. Note that, as shown in Figure 3.4 of section 3.4, a large fraction of main earners in low income households work less than 40 hours a week.

Next, we uncover further heterogeneity in the transmission of monetary policy to labour markets by estimating the effect on hours of employed persons seperately for individuals across different professions. I use the ISCO professional classifications, as reported in the SHP. Figure 3.9 shows the cumulative effect of a 10 basis points cut across professions on hours worked of employed individuals. The point estimates are largest for plant and machine operater assemblers, skilled agricultural and fishery workers and service workers/market sales workers. There are two points to note: First, these three are relatively elementary professions. Second, these three professions are associated with individuals from low income households, as shown in section 3.4. So the effects of monetary policy on the distribution of labour supply, which I have discussed so far, is associated with the heterogeneous effect on hours across different professions.



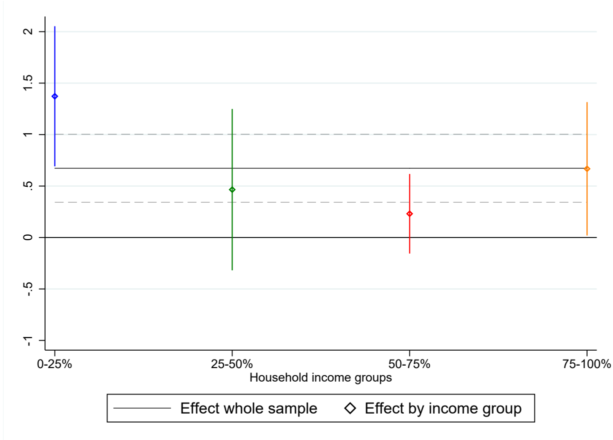
Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval across individuals with different professions. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is hours worked per week of individuals in work. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.9: *Effect of 10 bps cut across professions on hours worked per week*

Figure 3.25 in Appendix 3.7.3 shows the labour income effects analogous to Figure 3.9. The work income responses are comparable to the hours responses discussed above, suggesting an increase of labour demand among more elementary professions induced by expansionary monetary policy.

After establishing the effect on the intensive margin of labour supply, consider the effect on the extensive margin. First, I discuss the results of a policy rate cut in equation (3) estimated as a linear probability model, when the dependent variable is 1 if an individual is working and 0 if an individual is either non-active or unemployed. Figure 3.10 shows the cumulative effect 10 basis points cut from this model in percent across household income groups. Table 3.7 in Appendix

3.7.3 provides the results using non-linear probability models. The differential transmission across household income groups is similar to the one presented below.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is 1 if an individual is in work and 0 if an individual is either unemployed or inactive. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level. In total, the regressions include 34,712 observations.

Figure 3.10: *Effect of 10 bps cut across income groups on the probability of being employed in %*

All point estimates in Figure 3.10 are positive but only significant for individuals in the $0-25\%$ and $75-100\%$ household income groups. The results show that also in Switzerland, expansionary monetary policy promotes employment. The effect is largest in the bottom income quartile. Samarina and Nguyen (2019) find that in the euro area, expansionary monetary policy increases employment more in the European periphery, where employment is lower. A similar observation applies to the employment effects across Switzerland: For households with the lowest incomes (and the lowest employment rates), monetary policy has the strongest effect on the employment rate. Overall, this suggests that monetary policy transmits to hours via the intensive and extensive margin in those parts of the population, where the amount of hours worked of employed individuals and the employment rate is lowest. Note, however, that the employment rate as well as the rate of full-time employed reported in 3.24 in Appendix 3.7.3 also increases somewhat after a policy rate cut for the individuals in the top household income quartile.

While the transmission to the employment rate is interesting per se, the panel data set allows to further investigate these effects by looking at individual transitions between employment states. I distinguish between three different states: unemployment, out of the labour force and employment. I construct four dummy variables for transitions between employment states from last year to this year. I omit the transitions between unemployment and out of the labour force, because I focus on transitions that directly affect the employment rate. For individual i in quarter q and year t and employment state ε_{t-1} and ε_t, I define

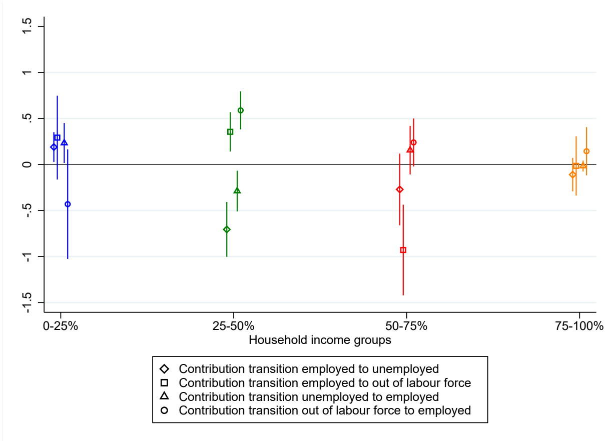
$$\text{Transition}_{iqt}^{\varepsilon_{t-1},\varepsilon_t} = \begin{cases} 1 & \text{if } i \text{ changes the employment state from } t-1 \text{ to } t, \\ 0 & \text{if } i \text{ does not change the employment state from} \\ & t-1 \text{ to } t. \end{cases}$$

I estimate equation (3) as a linear probability model for all household income quartiles with $\text{Transition}_{iqt}^{\varepsilon_{t-1},\varepsilon_t}$ as the dependent variable.

The effect of monetary policy shocks on the transition probabilities considered are hard to interpret per se, because their economic significance depends on the respective employment state shares in each household income group. In order to compute the impact on the income group specific employment rates, I multiply the cumulative effects with the respective share of individuals in the initial employment state in a given household income quartile.

Figure 3.11 shows the effect of a 10 basis points cut on the employment rate through the estimated effect on employment state transitions. Although the effect on the employment rate shown in Figure 3.10 for the top three household income quartiles is not statistically significant, the total effect masks offsetting transitions between employment states.

As an example, consider the estimated effect for individuals in the 25th to 50th household income quartile. While expansionary monetary policy increases net flows from employment to unemployment, this is offset by net flows from individuals from out of the labour force to employment.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1 and the respective share of individuals in the initial employment state in a given household income quartile. The dependent variable is 1 if an individual is moving from one employment state to another and 0 otherwise. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.11: *Effect of 10 bps cut across income groups on employment rate via employment rate transitions in %*

The effects in the bottom income quartile add to the findings in the literature that expansionary monetary policy works at the extensive employment margin, specifically by reducing unemployment. A policy rate cut is associated with a reduction of the probability to become unemployed and an increase of the probability of unemployed to become employed. This positive net flow from unemployment to employment is in line with theoretical predictions by Gornemann et al. (2021) as well as empirical evidence for the euro area (Samarina and Nguyen, 2019).

The results provide a novel perspective on the unique features of the Swiss labour market. As documented in section 3.4, the Swiss labour market is characterized by a high employment rate and few cyclical transitions between employment and unemployment. Although few cyclical transitions occur at the aggregate level, the transitions between unemployment and employment are important for households at the bottom of the income distribution, where the unemployment rate is highest. The heterogeneous effects of monetary policy across households shows that expansionary monetary policy affects the transition rates favourably for these households.

3.6 Conclusion

In this chapter, I have analyzed the distributional consequences and labour market effects of monetary policy in Switzerland. I find that expansionary monetary policy reduces income inequality between households. I have shown that the reduction is caused by an increase of household income and individual labour income at the bottom of the household income distribution. These effects are shown to have a spatial dimension. I argue that households in the Italian- and French-speaking areas of Switzerland gain most from a monetary expansion.

At the intensive margin, policy rate cuts increase the amount of hours worked for employed persons in low income households. Specifically, a 10 basis points cut leads to an increase of 0.9 hours per week in the bottom household income quartile. This increase is driven by individuals working in more elementary profession.

At the extensive margin, a 10 basis points cut leads to an increase of the employment rate in the bottom household income quartile of 1.4 percentage points.

Further, in the bottom household income quartile, policy rate cuts lead to an increase of transitions from unemployment to employment and to a decrease of transitions from employment to unemployment.

The findings suggest that there is substantial heterogeneity in the transmission of monetary policy. This heterogeneity provides insights on the mechanisms driving the transmission which is of interest for policy makers and economists alike. The estimation results also provide useful targets for structural models of monetary policy transmission.

3.7 Appendix of chapter 3

3.7.1 Monetary policy shocks

Table 3.4 shows the piece-wise correlation coefficient of monetary policy shocks, cumulated at the quarter, with lags of quarterly GDP and consumption expenditure growth rates. The fact that none of the correlations is statistically significant indicates that monetary policy shocks in Switzerland cannot be predicted by previous levels of economic activity.

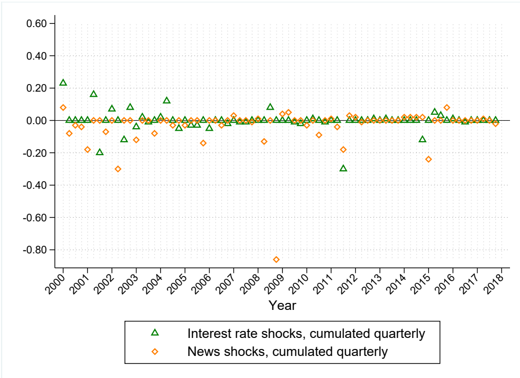
Table 3.4: *The correlation of lags of aggregate GDP and consumption growth with monetary policy shocks*

	GDP(-1)	GDP(-2)	GDP(-3)	GDP(-4)
Correlation with shocks	-0.0627	-0.0705	-0.0387	-0.1189
Significance level	0.6033	0.5621	0.7520	0.3343
	Consumption(-1)	Consumption(-2)	Consumption(-3)	Consumption(-4)
Correlation with shocks	0.0770	0.0701	0.1366	0.0885
Significance level	0.5233	0.5640	0.2630	0.4730

Sources: GDP and consumption growth rates from SNB (EPB@SNB.gdprpq{VVK,BBIP}, EPB@SNB.gdprpq{VVK,T0}). *Notes:* Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Correlation of quarterly monetary policy shocks with lagged quarterly growth rate of GDP and aggregate consumption expenditures, seasonally and calendar adjusted.

Figure 3.12 shows monetary policy shocks, cumulated at the quarter, and classified into news and interest rate shocks. The classification is done on the basis of the "poor man's" decomposition discussed in Jarociński and Karadi (2020).

Table 3.5 shows the results of a regression of GDP and consumption growth rates against three years of monetary policy shocks, quarter dummies and year dummies. The coefficients of monetary policy shocks have the expected sign, although only one of them is statistically significant.



Sources: Futures contracts' prices from TickDataMarket; series constructed as in Koeniger et al. (2021a). Stock market data (SMI) from Thomson Reuters. *Notes:* Interest rate shocks are monetary policy shocks that have the opposite sign as return of the SMI on the announcement dates. News shocks are monetary policy shocks that have the opposite sign as the return of the SMI on the announcement dates. In quarters without an interest rate or news shocks, the cumulated shock is zero.

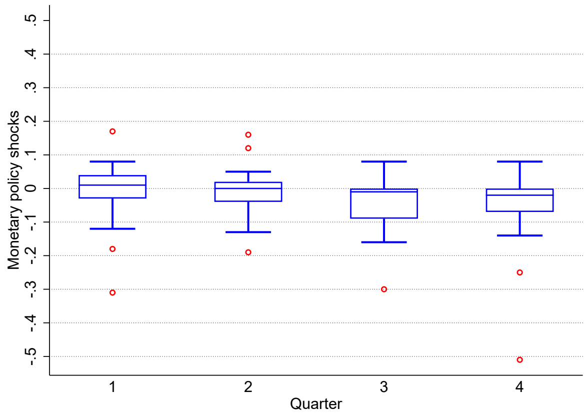
Figure 3.12: *Distinguishing between different monetary policy shocks*

Table 3.5: *The effect of monetary policy shocks on aggregate GDP and consumption growth*

	GDP	Consumption
Monetary policy shock, sum Q(-1,-4)	-0.281	-0.417
Monetary policy shock, sum Q(-5,-8)	-1.133	-0.826***
Monetary policy shock, sum Q(-9,-12)	-0.545	0.058
Quarter dummies	Yes	Yes
Year dummies	Yes	Yes
Observations	60	60
Cum. effect of 10 bps cut (in p.p.)	0.20	0.12

Sources: GDP and consumption growth rates from SNB (EPB@SNB.gdprpq{VVK,BBIP}, EPB@SNB.gdprpq{VVK,T0}). *Notes:* Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the quarterly growth rate of GDP and aggregate consumption expenditures, seasonally and calendar adjusted. Standard errors are clustered at the quarter of the interview, because the monetary policy shock does not vary at the household level. The cumulative effect over three years of a -10bps shock is obtained by multiplying the sum of the coefficients with -0.10.

Figure 3.13 shows boxplots of the monetary policy shock series across quarters. It documents that the monetary policy shock series does not exhibit systematically different patterns across quarters.



Sources: Futures contracts' prices from TickDataMarket; series constructed as in Koeniger et al. (2021a). *Notes:* The figure shows boxplots of monetary policy shocks by quarter. In total, there are 87 observations.

Figure 3.13: *Monetary policy shocks across quarters*

3.7.2 Descriptives

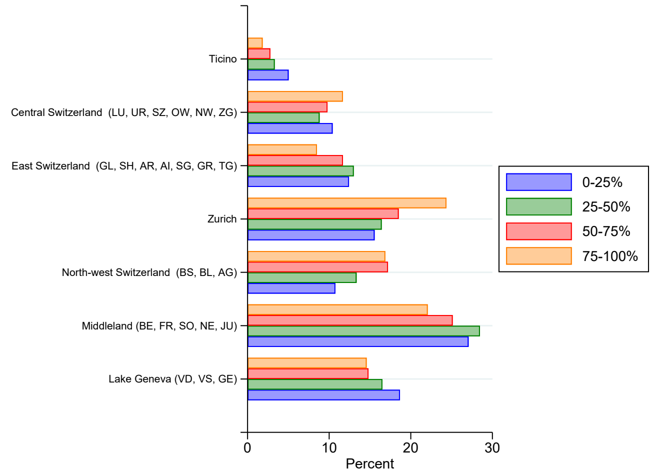
Table 3.6 shows the summary statistics by quarter for the full sample over all household income quartiles.

Table 3.6: *Summary statistics by quarter*

Quarter	1	3	4
Household characteristics			
Real gross total household income in eq. scales	88,558	92,383	96,213
Number of persons in household	3.0	2.9	3.0
Individual characteristics			
Real gross work income	71,295	67,273	74,619
Real social public transfer income	1,334	1,095	961
Real informal transfer income	2,030	2,015	1,890
Real income from other sources	7,399	7,796	7,224
Employment rate	88.1	83.7	88.3
Hours worked per week (>0)	38.9	33.0	35.0
Hours worked per week (>0, full time)	53.1	50.3	50.5
Full time (% of employed)	57.9	46.8	50.9
Age	42.9	47.8	46.5
Years of education	14.0	14.1	14.3
Female (%)	51.6	58.3	54.3
Married (%)	58.7	66.1	66.2
Has children (%)	63.6	70.0	69.8
Swiss nationality (%)	85.4	91.4	91.0
Observations	1,256	14,566	21,974

Sources: SHP. *Notes:* Summary statistics by interview quarter. Due to the survey setup, there are too few observations in the second quarter to produce meaningful summary statistics. The upper panel shows means for households interviewed between 2002-2017. The lower panel shows means for individuals interviewed between 2002-2017. Real incomes deflated by the CPI. See Appendix 3.7.4 for further details on the construction of the variables and the sample.

Figure 3.14 shows the distribution of regions across household income groups, illustrating which fraction of each household income group lives in which region. Households in the bottom quartile of the household income distribution more likely live in Ticino or the Lake Geneva region than the rest. Households in the top quartile more likely live in Zurich or Central Switzerland.

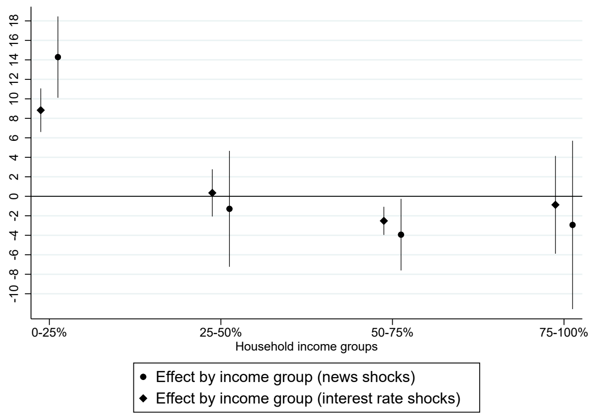


Sources: SHP. *Notes:* The figure shows the distribution of regions, the seven reported larger Swiss regions of households in the household income quartiles.

Figure 3.14: *Regions across household income groups*

3.7.3 Results

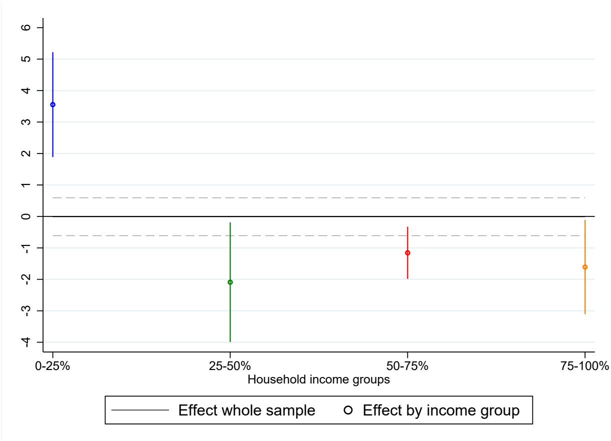
Figure 3.15 shows the results of separate estimations of equation (2) with log gross household income as the dependent variable and either interest rate or news shocks as monetary policy shocks. The fact that some shocks (news shocks) may convey private information about the state of the economy does not change the main results.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross household income in adult equivalents. $\mathbf{x}_{hqt}$ includes age and age squared of household's reference person. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the household level. Interest rate shocks are monetary policy shocks that have the opposite sign as return of the SMI on the announcement dates. News shocks are monetary policy shocks that have the same sign as the return of the SMI on the announcement dates. In quarters without an interest rate or news shocks, the cumulated shock is zero.

Figure 3.15: *Effect of 10 bps cut across income groups on gross household income in %*

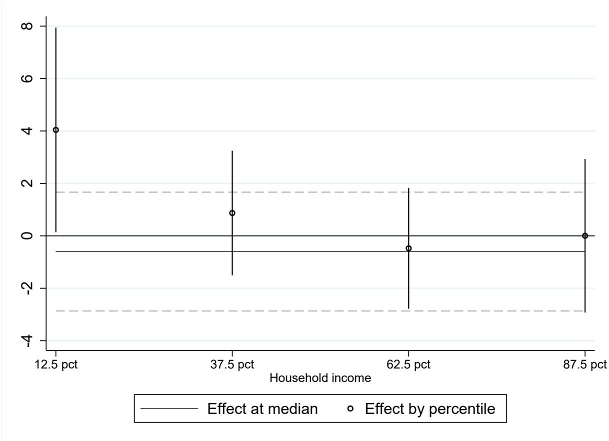
Figure 3.16 shows the results of separate estimations of equation (2) with log gross household income as the dependent variable and only two years of lagged monetary policy shocks. The resulting cumulative effects are somewhat smaller but qualitatively similar to the baseline.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross household income in adult equivalents. $\mathbf{x}_{hqt}$ includes age and age squared of household's reference person. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the household level.

Figure 3.16: *Effect of 10 bps cut across income groups on gross household income in %, using two years of lagged shocks*

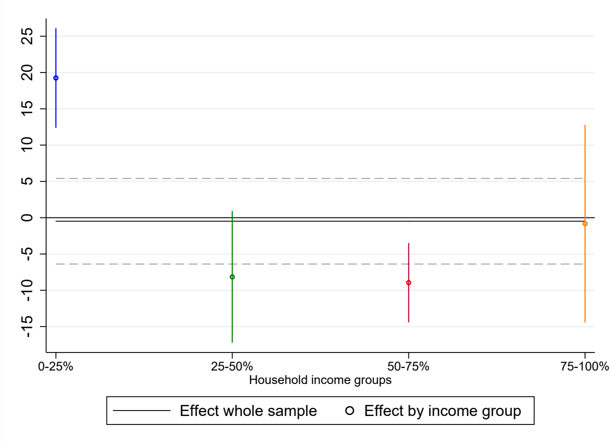
Figure 3.17 shows the results of quantile regressions with log gross household income as the dependent variable at the centre of household income bins.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval using a quantile regression at the middle of the household income quartiles. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross household income in adult equivalents. $\mathbf{x}_{hqt}$ includes age and age squared of household's reference person.

Figure 3.17: *Effect of 10 bps cut across income groups on gross household income in %, quantile regression*

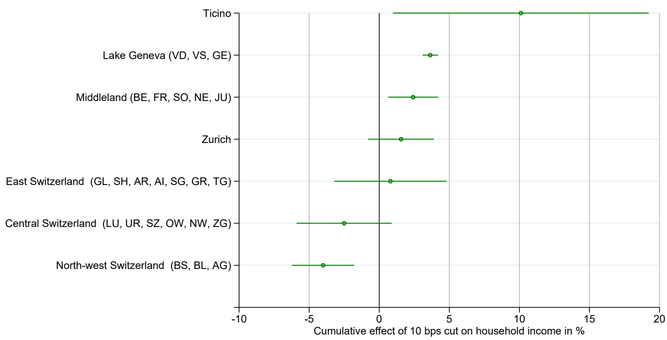
In the baseline specification, monetary policy shocks are defined as the day-to-day change in the implied rate of futures for the 3-month Swiss-Franc Libor on announcement dates. Figure 3.18 replicates Figure 3.6 with shocks defined using a shorter time window of six hours as in Corsetti et al. (2021).



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross household income in adult equivalents. $\mathbf{x}_{hqt}$ includes age and age squared of household's reference person. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the household level.

Figure 3.18: *Effect of 10 bps cut across income groups on gross household income in % using shorter time window around the announcements*

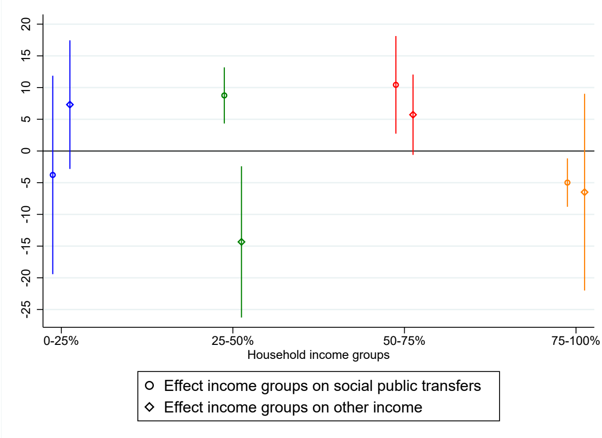
Figure 3.19 shows the cumulative effect of a 10 basis point cut on household income. Households are categorized by their NUTS2 region at every observation. The regression is estimated separately for each classified NUTS2 region.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross household income in adult equivalents. $\mathbf{x}_{hqt}$ includes age and age squared of household's reference person. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the household level.

Figure 3.19: *Effect of 10 bps cut across regions on gross household income in %*

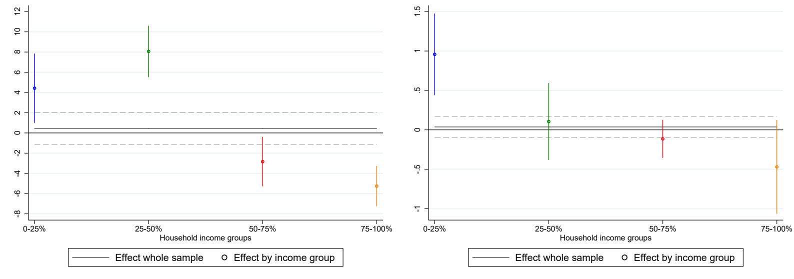
Figure 3.20 shows the cumulative effect of a 10 basis point cut on other individual incomes.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly individual income from social public tranfers (circles) and the log of real yearly individual income from other sources (diamonds). $\mathbf{x}_{hqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.20: *Effect of 10 bps cut across income groups on other incomes in %*

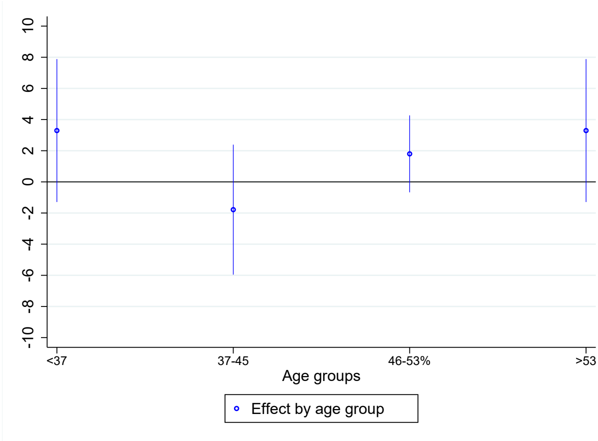
Figure 3.21 shows the results of the main regressions using individual data controlling for individual characteristics. The left panel of Figure 3.21 shows the cumulative effect of a 10 basis point cut on work income of employed individuals. The right panel of Figure 3.21 shows the cumulative effect of a 10 basis point cut on hours worked per week of employed.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly individual work income of employed individuals (left) and hours worked per week of employed indiviudals (right). $\mathbf{x}_{hqt}$ includes age, age squared, years of education, and dummy variables for the gender, marriage status, children, and Swiss nationality of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.21: *Effect of 10 bps cut across income groups on work income of employed individuals in % (left) and on hours worked per week of employed (right) with additional individual controls*

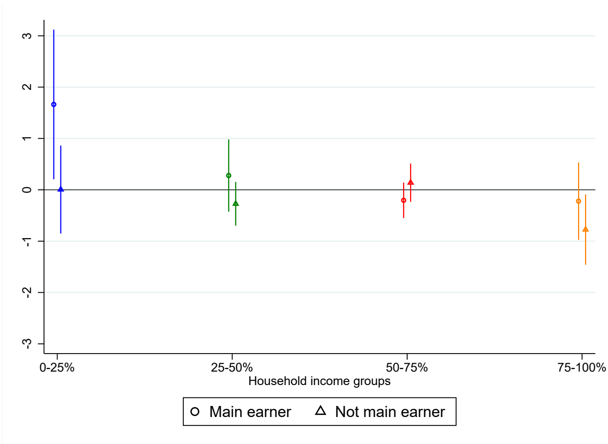
Figure 3.22 shows the cumulative effect of a 10 basis point cut on yearly individual work income of employed individuals for four equally sized age groups.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly individual work income of employed individuals. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.22: *Effect of 10 bps cut across age groups on work income of employed individuals in %*

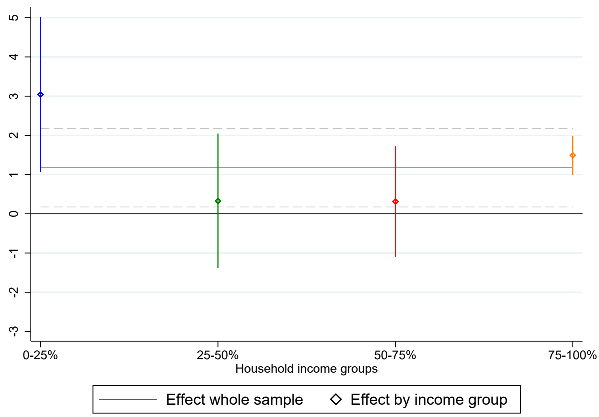
Figure 3.23 shows the cumulative effect of a 10 basis point cut on hours worked per week of employed for regressions run separately for household main earners and other household members.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is hours worked per week of individuals in work. A household's main earner is defined as a household member who earns at least half of total gross household income with her labour income. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.23: *Effect of 10 bps cut across income groups on hours worked per week of employed distinguishing main earners and other working age adults in the household*

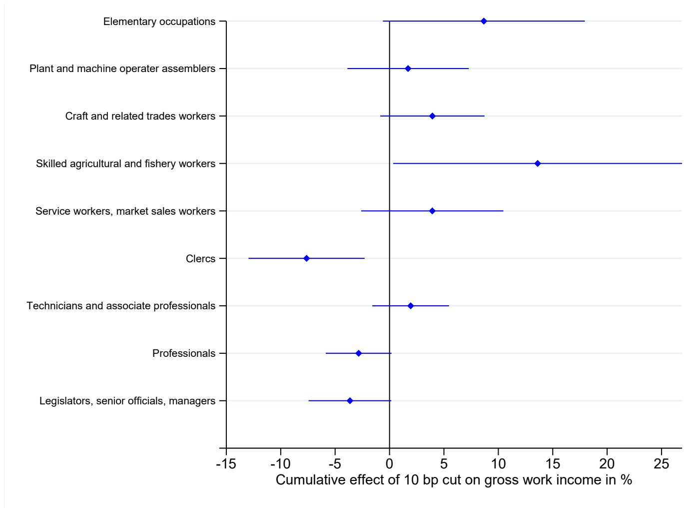
Figure 3.24 shows the cumulative effect of a 10 basis point cut on the rate of the full time employed over employed.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is 1 if an individual is in work and reports to work full time and 0 if an individual is in work and reports to work part time. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.24: *Effect of 10 bps cut across income groups on rate of full time employed in %*

Figure 3.25 shows the cumulative effect of a 10 basis point cut on work income of employed for regressions run separately for each reported profession.



Notes: The graph shows the cumulative effect over three years of a -10bps shock together with the 95% confidence interval. The effect is obtained by multiplying the sum of the coefficients with -0.1, so the sum of the elements of $\hat{\beta}$, multiplied by -0.1. The dependent variable is the log of real yearly gross work income of individuals in work. $\mathbf{x}_{iqt}$ includes age and age squared of the individual. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the individual level.

Figure 3.25: *Effect of 10 bps cut across professions on work income of employed in %*

Table 3.7 shows the effect of monetary policy shocks on the employment rate with non-linear specifications. The results indicate a strong and significant effect in the bottom household income quartile confirming the results of the linear probability model in Figure 3.10 in section 3.5.

Table 3.7: *The effect of monetary policy shocks on the employment rate with non-linear specifications*

	0-25%		25-50%		50-75%		75-100%	
	Probit	Logit	Probit	Logit	Probit	Logit	Probit	Logit
Monetary policy shock, sum Q(-1,-4)	-0.0881***	-0.0840***	0.0181	0.0194	-0.0231	-0.0191	0.00170	-0.000459
Monetary policy shock, sum Q(-5,-8)	-0.0912***	-0.0834***	-0.0192	-0.0158	-0.0157	-0.0155	-0.0401***	-0.0396***
Monetary policy shock, sum Q(-9,-12)	0.0313*	0.0316**	-0.0604*	-0.0555*	-0.00804	-0.00778	-0.0288**	-0.0291*
Quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,697	6,697	8,511	8,511	9,246	9,246	10,258	10,258

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The reported coefficients are marginal effects in the logit and probit specifications at the mean of the independent variables. The dependent variable is the 1 if an individual is working and 0 if the individual is either unemployed or not in the labour force. Individual controls includes age and age squared of household's reference person. Standard errors are clustered at the year-quarter, because monetary policy shocks do not vary at the household level.

3.7.4 Data Appendix

The Swiss Household Panel (SHP) is an unbalanced household panel that starts in 1999. Households and individuals are interviewed once a year. For each survey year, the information on households as well as individuals are merged.

I drop individual observations with negative individual total gross income or negative individual labour income. Additionally, household members of households with negative gross household income are dropped. I keep individuals at working age, so those that are older than 20 and younger than 65 years. For the household regressions (equation (2)) I only consider quarters where there are at least 200 observations to contain the effect of outliers.

Table 3.8: *Variables used*

Variables at the household level		
Name	Variable name in SHP	Description
Region	regionW	Region of residence
Number of persons in household	nbpersW	Number of persons in household
Yearly gross total household income	iWhtyg	Yearly household income, gross
Variables at the individual level		
Name	Variable name in SHP	Description in SHP
Yearly gross work income	iWwyg	Yearly work income, gross
Yearly social public transfer income	iWstpy	Yearly social public transfer income
Yearly income from other sources	iWosy	Yearly income from other sources
Working status	wstatW	Working status
Hours worked per week	pWw610	Total number of hours worked in all jobs
Work schedule (full time/part time)	pWw39	Current main job: Part-time or Full-time
Profession	is1majW	ISCO classification: Main current job: 1-digit-position
Age	ageW	Age in year of interview
Gender	sexW	Sex
Civil status	civstaW	Civil status in year of interview
Education years	edyearW	Years of Education based on ISCED Classification
Swiss nationality	nat_1_W	First nationality

Sources: SHP. *Notes:* Variables used from the Swiss Household Panel. For further details, see the documentation of the SHP in Voorpostel et al. (2017).

Table 3.8 summarizes the variables used from individual as well as household data sets. The first column provides the variable names used in this chapter. The second column shows the variable code in the SHP, with "W" indicating the wave number. The third column shows the name of the variable in the SHP questionnaires.

To convert household incomes into adult equivalents, I use the mean of equivalent scales of Fernández-Villaverde and Krueger (2007) in Table 1, column 7 (p. 554). For household sizes greater than 5, an additional 0.3 is added for each person. Incomes are deflated by using the annual nationwide CPI indexed to 2010 (FRED code *CHECPIALLMINMEI*).

For Figure 3.9, I consider hours worked in the current main job, because the regression conditions on individual professions in the current main job.

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